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THE

AMERICAN

JOURNAL OF PSYCHOLOGY

EDITED BY

G. STANLEY HALL
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VOL. XXVII

WORCESTER, MASS.
FLORENCE CHANDLER, Publisher
1916

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THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded by G. Stanley Hall in 1887

Vol. XXVII JANUARY, 1916 No. 1

DURATION AND THE TEMPORAL JUDGMENT[1]

By Josephine Nash Curtis

Introduction

The tendency of modern text-books of psychology has been to abandon the concept of duration as a derived character of mental processes or as a relation of psychical elements, and to adopt in its place the view that duration is an attribute of sensation co-ordinate with quality and intensity. The basis for this change has been in the main logical and theoretical; but the question must arise whether we can obtain any introspective evidence for the ultimate character of duration. There are, of course, two possible sources of such evidence: incidental references to the matter in the experimental literature, particularly that of the 'time-sense'; and introspective data obtained by further investigation which has, as the older experiments had not, the question of the ultimate character of duration as its goal.

Study of the literature of the subject proved less fruitful than might at first be imagined.[2] The first of the experimental studies to discuss the nature of duration is that of Mach,[3] who says that all sensations are accompanied by the time-sense. He attempts to reduce this 'time-sense' to attention, and places himself on the side of those who believe the temporal element is not an ultimate datum. In his later writings,[4] Mach upholds a definitely genetic theory of time, and so denies that duration may be an attribute of sensation. Vierordt[5] remarks that "the operations whereby we gradually come to a concept of the temporal and the spatial, and which allow us, even compel us, to add to our sensations the measure of time and space are completely hidden from us," and that "the spatial and temporal dimensions of sensory stimuli come, so to speak, immediately into our consciousness." Fechner[6] maintains that "a longer time between two impressions appears longer not on account of its abstract, empty, physical size, but on account of the greater sum of psychophysical activity which it includes," thus indicating that duration is to be considered neither as mere physical duration, nor as an immediately given character of mental process.

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[1] From the Psychological Laboratory of Cornell University.

[2] The writer read, beside the references given in E. B. Titchener, Experimental Psychology, vol. II, part ii, 1905, 394-400, the more recent work such as that of Benussi, Yerkes and Urban, Urban, Overstreet, Rageot, Burrow, Lobsien, Peres, Schmied-Kowarsik, Brewer, Gildmeister, Stratton, Wallin, Biehle, Dunlap, Pitkin, and others listed in the current bibliographies, beside many articles which proved to be concerned with the philosophical, musical, or physiological, rather than with the psychological aspect of our problem.

[3] E. Mach, *Wiener Sitzungsber.*, math.-naturw. Cl., 51, Abth. 2, 1865, 133; *Die Analyse der Empfindungen*, 1902, 185ff.

Münsterberg,[7] after many experiments performed upon himself, concludes that our time sense is manifested in a hundred forms, but that all these forms have strain sensation as basis. For short intervals he finds rhythmic tensions of the muscles about the sense organs the basis of judgment. For all intervals, however, the processes accompanying respiration primarily determine our estimation of time. Nichols[8] concludes that duration is an attribute of every sensation and every image, and that this attribute is the ultimate and essential datum of time, although his conclusion seems little connected with his experimental work. He distinguishes between perception and apperception of time.[9] Time, for him, is apperceived "when any process of duration occupies the focus of attention, is the object of association, and calls up durative associations," and it is only as associated ideas of length that we have the muscular tensions, dermal stretchings, joint pullings, etc., that have been taken by others to be the sole components of the perception of time. Münsterberg and Wylie[10] confirm Münsterberg's earlier work by finding that the "subjective measure for time lengths seems to lie in sensations peripherally aroused by muscular activity, especially by the strains and relaxations which take place in the various groups of muscles conditioned upon bodily reactions to changing intensities of stimuli. Such reactions occur in the functions of breathing, in the voluntary movements of the eyes, limbs, etc. Stevens[11] also denies the immediacy of temporal judgments. He writes "our judgment of time is mediate, depending upon organic processes, of which change in blood volume is one of the more important." Aliotta[12] names rhythm and expectation as two criteria in judging times, and says that the former is more effective for short times, the latter for long, but he does not go into detail. Up to this point the writers, with the exception of Vierordt and Nichols, have insisted on the secondary character of temporal judgments. Meumann[13] now carries out a long series of experiments, the result of which attests the immediate character of these judgments, and apparently the ultimate character of duration. He works under Wundt's direction, and the system in terms of which he naturally expresses his ideas makes duration a creation of the synthesis of sensation with feelings of expectation and fulfillment. Nevertheless he writes: "According to my notion *every* psychical process is measure of time in the sense that every psychical process brings to consciousness, through its qualitative and intensive *changes,* temporal relations of *succession;* and through its relatively *unchanged persistence* in respect to quality and intensity, temporal relations of *duration.* . . . Whether or not all psychical processes can bring temporal relations to consciousness in an equally *accurate,* and—for our apprehension— an equally *convenient* way, or whether we perhaps *favor* . . . certain psychical processes to make the temporal relations clear, just as we depend on the eye for the estimation of space, although we could use the hand or arm, is quite another question."[14] He goes on to say: "I presuppose as a final fact of experience, incapable of further discussion, that the processes of our consciousness, of our inner perception, are given constantly at the same time as processes of a *temporal* nature, i. e., as standing in a temporal relation, and that we can bring these temporal relations of our conscious processes to consciousness in relative abstraction, just as we can perceive relations of intensity in comparative isolation, although they are always there merely as the intensive steps of certain qualities. I shall further take it for granted that this temporal state, which is brought to consciousness in relative isolation, can be made the unique object of a judgment."[15] He then distinguishes between mediate and immediate judgments of time, and urges that both kinds occur. From the observations of his subjects, as well as from his own more or less theoretical conclusions, he argues for the immediacy of temporal judgments. "Since it is so difficult to compare two differently filled times, the observers easily hit upon artificial aids to judgment, thinking thereby to make the judgment easier. This course, however, I always prohibited, and requested a *purely passive resignation to the impression of the interval of time.* It is interesting to note that in the course of the experiment all such aids as beating time, nodding the head, respiratory movements, innervation of the larynx, rhythmical counting, and so on very soon disappeared. They were recognized by the observers as disturbances, rather than as aids." Meumann, then, states definitely his belief in the immediate nature of the judgment of time, and he seems to imply also that duration is an ultimate datum of time. He speaks, to be sure, of 'temporal relations,' but quite as he might speak, if he liked, of 'intensive relations' rather than of intensity. His comparison of 'temporal relations' with intensity, and his approval of Nichols' statement that duration is an irreducible attribute of sensation (when Meumann quotes Nichols (*ibid.,* 8, 1892, 503) he speaks of the "elementary, irreducible time experience," though Nichols himself says "attribute of every sensation and every image") seem to place Meumann in the group of those believing in the ultimate nature of duration.

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[4] *Erkenntnis und Irrtum, Skizzen zur Psychologie der Forschung,* 1906, 423ff.

[5] K. Vierordt, *Der Zeitsinn nach Versuchen,* 1868, 13f.

[6] G. T. Fechner, *Abh. d. kgl. sächs. Ges. d. Wiss.,* Math.-phy. Cl., 13, 1884, 7.

[7] H. Münsterberg, *Beitrage zur experimentellen Psychologie,* II, 1889, 20.

[8] H. Nichols, *Am. Jour. of Psych.,* 4, 1891, 85.

[9] *Op. cit.,* 92ff.

[10] H. Münsterberg and A. R. T. Wylie, *Psych. Rev.,* 1, 1894, 52.

[11] H. C. Stevens, *Am. Journ. of Psych.,* 13, 1902, 26.

[12] Aliotta, R., Instituto di Stud. sup. di Firenze, *Ricerche di Psicologia,* 1, 1905, 1ff.

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[13] E. Meumann, *Phil. Stud.,* 8, 1892, 450ff.

[14] *Op. cit.,* 504.

[15] *Phil. Stud.,* 12, 1896, 143.

Now, again, denial of the final nature of duration appears. Schumann[16] says: "A secondary impression of suspense of expectation is noticed before every impression, though I can not determine on the basis of introspection whether it consists of strain sensations aroused by muscular contractions, or of internally aroused feeling of strain. The secondary impressions of expectation and surprise are really the psychic contents on which we rely in comparing short times." Elsewhere,[17] however, he acknowledges that there may be another psychical element present in time-comparison which has escaped notice, and finally[18] he concludes that although accurate and subjectively certain judgments are always mediate, the question of whether we can ever judge two intervals immediately "is not wholly simple to decide." Quandt[19] is not definite as to the true nature of the judgment, but asserts that "estimation by secondary peripheral accompaniments on which Münsterberg has sought in greater part to base his theory of time-perception may on no account be considered conclusive." Katz'[20] observers find that in general certain strain sensations seem to be essential to the experience of time. Katz finds judgments like those of Schumann in which the observer expects the second member to equal the first and judges whether this member stops too soon, at the proper time, or too late to equal the first, and he also finds judgments of absolute impression. Further discussions of these forms of judgment and of Katz' criticism of Meumann's passive instructions will be given later in this paper. Alvord and Searle[21] found that in general judgments are made on the basis of strain and relaxation, imagined movements, auditory rhythm and the spontaneous occurrence of auditory images of the stimulus, although some intervals were said by one observer to be judged 'mechanically.' Schultze[22] speaks of the spatial, intensive, and qualitative characters of "experience and mental structures" and concludes that temporal extent is a fourth such character which cannot be reduced to the others. Moede[23] instructs his observers to give "free" judgments. From their reports he finds two basic constituents (*Grundbestandteile*)[24] of every temporal consciousness: duration and progression (Fliessen). "At one time the observer may give himself up to temporal course and is then taken up with the progression of the quality. At another time he may lift himself out of the level of the continuum and look out from a higher, as it were resting, point of view. In the one case he is like the boat in the stream, which is carried on by the waves, whereas in the other he is like the viaduct through which the stream of temporal quality runs. The natural set is a mixture of the extremes, which can best be described as *progressing duration*."

Let us now see what arguments are urged as showing that duration cannot be an original character of sensation. There are two main arguments against the attributive character of duration. The first is the argument of the Wundtian school, —the argument from relativity. Statements of duration, it is said, can only be relative. Any temporal experience whatever implies logically a relation to other experiences; to be durative at all, experience must be apprehended as longer than, shorter than, or equal to another, before or after or simultaneous with another; and an experience which merely lasts, but does not last for a definite time, or at a definite time, is unthinkable. Psychologically, this objection takes the form of stating that duration is a perception, the character of which has developed from the interplay upon sensation of other processes, such as strain sensations or the various feelings which are connected with the sensation in experience. Duration, accordingly, must be regarded not as an original character, an attribute of sensation, but as the final product of a creative synthesis. Such an objection can hardly be met by direct introspection, by observation pure and simple. But if we find cases in which there appears an approximation to a duration which introspectively is neither longer, shorter, nor equal with regard to another, we shall go some way toward meeting the logical argument.

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[16] F. Schumann, *Zeits. f. Psychol.*, 4, 1892, 2.

[17] *Ibid.*, 17, 1898, 113.

[18] *Ibid.*, 125.

[19] J. Quandt, *Psych. Stud.*, 1, 1906, 153.

[20] D. Katz, *Zeits. f. Psychol.*, 42, 1906, 302ff.

[21] E. A. Alvord and H. E. Searle, *Am. Journ. of Psych.*, 18, 1907, 177.

[22] F. E. O. Schultze, *Arch. f. d. ges. Psych.*, 13, 1908, 275.

[23] W. Moede, *Psych. Stud.*, 8, 1912-13, 327.

[24] *Ibid.*, 366.

The second argument against the attributive character of duration makes a more direct appeal to introspection.[25] "Suppose, then, that we allow sensations a psychic duration, as distinguished from objective duration. Whatever the character of this consciousness, it must be contained in the 'psychic present,' else surely we are no longer dealing with a single element. Within this limit, then, sensations must be capable of differing in subjective duration. But can the 'psychic present' have subjective duration? . . . The psychic moments follow each other; they have no duration. The perception of duration is the perception of two or three such successive moments having similar contents. . . . While it seems absurd to treat their objective duration as one of the attributes of sensations, yet the only other way to maintain that they have a temporal attribute is to hold that the subjective present does possess subjective duration, a view which I think introspection does not confirm." The proving or disproving of the attributive character of duration, then, according to this view, lies in the introspective question whether a sensation may be perceived as duration within the limits of the conscious present.

From the results of our experimental work we shall endeavor to draw two main conclusions. First, that the results of previous investigations are not as contradictory as they at first seem; secondly, that the two chief arguments against the ultimate nature of duration are not supported by experimental observation.

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[25] M. F. Washburn, *Psych. Rev.*, 10, 1903, 421ff.

Experimental Investigation

Purpose: As already stated, the main object of this series of experiments was to discover what evidence introspective analysis offers for the attributive nature of duration.

Stimuli: Following the example of many earlier investigators, we used auditory stimuli throughout the first groups of experiments.

Apparatus: The length of the stimuli was controlled by a device attached to the Leipsic 'time sense' apparatus.[26]

This device consisted of four aluminum arms clamped to the central shaft of the rotating disc, and so arranged that, when two of the Leipsic lever-contacts were in position, the first arm moved the one lever of the first contact the second arm the other lever. The third and fourth arms acted similarly upon the second contact. In order that a given pair of arms should not act upon both contacts, it was necessary merely to raise one contact slightly and to raise the corresponding arms. By this arrangement the first two arms moved the first contact and passed under the second; the other pair moved the second contact similarly, but passed over the first without touching. In order to prevent the single member of a pair of arms from moving both levers of a contact, a small copper post was erected upon one lever of each contact, a short distance from the point of the lever, and the corresponding arm raised slightly and given an appropriate length. With such arrangements, when the Baltzar kymograph, to which the whole apparatus was attached, revolved, the disc of the time apparatus revolved also and each arm moved only one lever of one contact. Further advantage of the aluminum arms lay in the fact that they were so attached to the shaft that either the first arm or the fourth arm might be shifted in position without altering the positions of the other three. It was thus possible to vary the length of the first, of the second, or of both members of any pair of stimuli. The sound itself was given by an electrically driven 254 v. d. tuning fork. The fork was placed in a sound-proof box with its resonator opposite a telephone transmitter. The transmitter and a dry-cell were connected with the primary coil of an inductorium; the contacts of our apparatus and two telephone receivers, one for experimenter and one for observer, were connected with the secondary coil. A switch was introduced in such a way that the experimenter could hear the sound in the receiver whenever the kymograph revolved, while the observer heard the sounds only when the switch was closed. The tone which the observer heard was of moderate intensity, and was without unpleasant buzzing of the receiver. At first the kymograph was tested every two or three weeks; but after a number of trials it was found that the average deviation for ten readings was never more than .02 sec.; so that in the later work the apparatus was tested only at the beginning of a new group of experiments. A series of readings taken to find with how great accuracy the experimenter usually set the instrument showed a mean deviation of .03 sec. The observer was placed in a quiet dark room and heard the sounds through a telephone receiver attached to his left ear. This dark room and the room of the experimenter were connected by a system of electric bells, which enabled warning signals and reports of judgment to be sent from the one room to the other, as well as by a speaking tube through which the observer gave his introspections.

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[26] A diagram and detailed description of the 'time sense' apparatus may be found in W. Wundt, *Physiologische Psychologie*, 2, 1893, 424, and in Meumann, *Phil. Stud.*, 9, 1894, 270.

Method: The psychophysical method used in all the experiments was that of constant stimuli. The series were from 12 to 16 members in length, and in all except the first group (in which irregular steps were used) the steps were of .1 sec.

General procedure: The experimenter started the kymograph, allowed it to rotate twice to attain its maximal speed, rang two warning bells[27] 4 sec. and 2 sec., respectively, before the first sound was to be heard, closed the switch, and opened it again after the second sound had been given. The kymograph was then stopped, the judgment which the observer had rung on his bell was recorded, a new setting was made, and the procedure was repeated. The time from the ringing of a judgment to the first warning bell for the next pair of stimuli was approximately 45 sec.

Treatment of results: Our chief interest lay in the introspections, and no claim is made for the significance of the numerical results. There are in general entirely too few cases in any group to have any importance, and no attempt has been made to make the quantitative series complete, i. e., to compensate for time-error, etc. All the limens have been calculated by the method of right and wrong cases. In the mathematical work, Urban's table and Crelle's three-place multiplication tables were used. All our results are recorded in the number of degrees on the instrument (50°=1 sec).

Group 1

This group consists of preliminary experiments, whose general purpose was to find what secondary criteria were most likely to come into the judgment of time-intervals even when the observers were warned against such criteria, and to obtain hints for the direction of further work. Two continuous sounds were given with an interval between them of 1.5 sec. Three standard times were used: 50°, 75° and 100°, given in both time orders.[28]

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[27] Observers Bo and D, and later Gou and W preferred one warning bell and there seemed no reason for insisting on two being given.

[28] Only one contact was used on the apparatus.

Observers: Dr. E. G. Boring (Bo), instructor in psychology, observed 7 hours; Mr. F. L. Dimmick (D), graduate student, 15 hours; Dr. W. S. Foster (F), instructor in psychology, 20 hours; and Dr. M. E. Goudge (Gou), graduate student, 30 hours. Bo, F, and Gou were highly practised observers; D was relatively inexperienced.

Instructions to observers: "About two seconds after the second warning signal you will hear two sounds in the telephone receiver. You are to judge whether the second of these is longer than, equal to, or shorter than the first. Give your judgment with the push-button as follows: shorter than, 1 ring; equal to, 2 rings; greater than, 3 rings. If it should be necessary to give a doubtful judgment, give two short rings after the rings for the judgment. You are warned to be on the lookout for secondary criteria, and especially not to get in the habit of judging by some secondary criterion."

Results: The quantitative results of this group are so scanty as to be wholly without significance.

From the introspections[29] we find, as Meumann did, that at first all sorts of 'artificial' aids are employed in making the judgment.

The most commonly used aid is *kinaesthesis* in some of its manifold forms. Bo reports at first that "chest kinaesthesis has a great deal to do with it. I get a very definite muscular feeling as if I were pressing down inside my chest. It is quite independent of breathing; is probably imaginary. These processes carry the course of the sound; by that I mean that during the sounding of the second member, I assume the attitude of making the noise voluntarily myself, i. e., I screw this chest pressure down as if it were making the noise." At a later date Bo says: "There always seems to be relevant kinaesthesis in the body somewhere."

At first D, also, reports kinaesthesis: "Carried the first over sort of kinaesthetically," "sort of hummed along with both tones, then decided whether the second felt longer, shorter, or equal," and "judgment passed according to the kinaesthesis."

Gou used in general eye-kinaesthesis. She reports: "images representing the length of the two members, consisting for the most part of kinaesthetic sensations in the eyes, meaning 'length of tone as represented by a line which I couldn't see, but which I localised.'"

Early in the experiments F stated that "the sound comes, is attended to clearly, the head goes forward slowly just a bit, keeps moving forward all the time the sound is there, at the end it comes back with a sudden jerk. With the second sound, the head goes forward again just the same sort of way and snaps back at the end," and "when the second sound had gone on as long as the first, which I think also means, when the head movement had gone on as long and as far as the first head-movement. . . ." After some practice, F reports that the body and head kinaesthesis disappeared.

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[29] The excerpts from the introspections which are quoted are, of course, very scanty, but they are representative. The complete introspections are left in the Cornell psychological laboratory.

When warned that the use of kinaesthesis might be interpreted as constant reliance on particular secondary criteria, D[30] reports that the kinaesthesis is not necessary for the judgment: "don't think there was any kinaesthesis here, though the judgment seemed just as easy to make."

Special instructions were given to Gou to do away with kinaesthesis entirely, but this proved to be impossible. She says: "I can't just have tone sensation and nothing else in consciousness unless I give myself up passively, and then I am not judging anything."

Bo observed for so short a time that it is impossible to say whether with further practice he would have discarded kinaesthesis, or whether he would have been able to do so under instruction.

All observers report at times supplementary "representations," or "formulations," of the sounds. In general these are most frequent in the first stages of the experiments, and when there is some doubt about the judgment. They usually appear either between the members or after the second member. They are found in various sense-departments, may arise involuntarily or be called up voluntarily, and may be either detailed or abbreviated.

Bo says: "I formulated the length of the first member just about at its end. By formulation I mean that there seems to be some way in which the length of the member gets itself represented. I don't think this representation is a temporal course; if it is, it is a very much abbreviated one. I can't describe the thing which carries the length of that member, but I think it is like kinaesthesis"; "the formulation involved auditory imagery;" "the formulation is the whole sound in a nut-shell (running off in perhaps 1/50 of the time of the member), temporal course and all; some chest kinaesthesis, some ear kinaesthesis, and some auditory image." Bo also speaks of "visualising the members as running along together."

D speaks of "visualising the two sounds as a couple of gray streaks," "carrying the first sound over kinaesthetically," and one day, "visual images seem to mean the sound, but I don't think I judged the lengths of the sound by them."

F reports various kinds of representations: "went over the whole thing in imagery (visual to a great extent) in some short-hand way"; "the going over in imagery has much more kinaesthesis in the throat than much of my ordinary imagery of sounds"; "went over the whole experiment in image, mostly auditory, which I don't think took as long as the actual experiment"; "intentionally ideated the thing schematically, both auditorily and visually"; "there was a representation which, as nearly as I can tell, was an auditory image of the end of the tone over again with some realization that there was more ahead of it and how much more there was ahead of it. What that realization was I simply don't know;" and "this time it was a real representation, sometimes it is symbolic."

Gou speaks of a "fixing" of the duration of the first member in the interval, "by means of kinaesthetic terms; i. e., kinaesthetic sensations in the eye-region and trunk of the same kind as had accompanied the first member were present and meant 'first member was just so long'"; "very vague visual images representing the length of the two members"; "sometimes I choose between comparing quickly in schematic form and, as it were, living the experience over again"; "some kind of a representation of the member in scrappy and schematic form, in a nut-shell, one might say."[31]

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[30] D's later introspections, however, lay such stress upon the kinaesthetic and organic accompaniments of the comparison, that it seems possible that in these early experiments, before he attained practice in introspection, he may have overlooked vague or weak strains, etc., which he noticed later.

In the method of forming the judgment we find several tendencies which agree more or less with the findings of earlier investigators. The most common tendency is expressed sometimes as "expecting the second to be just like the first" and sometimes as "laying the first off on the second."

Bo says: "It was almost as if the second itself meant the first, as if there were identity"; "I reformulated the first upon the second as a background. . . . I held the formulation as a sort of a static kinaesthetic thing with kinaesthetic kinks in it on the background of the smooth second tone. Then I cut loose from the formulation and anticipated the end of the second member. This anticipation was a sort of heightening of the kinaesthetic complex in which one kink became especially intense and in which reference forward was visual image or eye-movement (I don't know which). It was as if out of the complex I got a cue relevant to the end, as if I were set ready to 'go off'; the muscular feeling of an incipient action. The tone ran on, and just as it stopped the kinaesthesis changed as if I had 'gone off.' Then a moment's hesitation, then retrospection, mostly visual, of the tone and the kinaesthesis snapping off together, and the judgment of equality." "Some time after the second member had started, I voluntarily recalled the first member and started it running along with the second. Was slightly surprised to find they fitted although they hadn't begun at the same time, because I thought the formulation had to run a course that ran along with the second member and couldn't be started late."

D speaks of "carrying the first member over to the second largely as a kinaesthetic image in my throat" "this time the sounds seemed very much alike, seemed like the same one over again."

F reports: "In some way I identified the second sound with the first;" "I attend to the second tone as if it were the first tone being repeated. In a certain sense I expect the second tone to be the same as the first. . . . This attending to the second the same way I attend to the first, this expecting the second to be the same as the first, this taking the second tone as a repetition of the first, means that the end of the second tone is somehow expected to come at a definite place ahead"; "the second is apprehended as a repetition of the first. There seems to be no conscious process to carry the meaning that it is a repetition, it merely is a repetition."

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[31] These reports of 'short-cut" images and sensations which adequately represented the time-intervals are in direct contradiction to Nichols, who writes (*op. cit.*, 90): "I think that every one who will observe his own mental processes when he seeks to measure or to realise the length of any durating sensation or its representation in memory will easily observe that he never fully perceives or remembers the length instantly or even approximately so; unless, of course, the duration is itself instantaneous or approximately so. On the other hand, I think anyone will easily convince himself that *fully* to perceive or to remember the length of its representation, these representations must stretch themselves out through an equal process and lapse of time as did their original occurrence."

Gou did not state definitely that she ever took the second as a repetition of the first or that she "laid off" the first on the second.

A second type of judgment that began to appear near the end of this group of experiments was that called variously "automatic," "immediate," or sometimes "unconscious."

Even during the short time for which Bo observed he reports once that the judgment was "almost automatic," and several times that the ringing of the judgment was "unusually automatic, as I didn't think definitely of giving three rings until I was actually hearing the second."

D reports a number of times: "I knew it was shorter the minute the sound stopped"; "that was almost automatic, as I was thinking about something else"; "the deciding seems to me to be unconscious"; "I just hear the sounds and make the judgments without knowing why or how."

F gives many of these judgments: "end of the sound and start back of the body came at the same time. Automatically pushed the button twice"; "judgment touched off automatically, don't get internal speech or anything like that, just press button"; "automatically reacted with three very quick and strong pressures and was surprised. Haven't any idea why I pressed the button three times. Judgment was entirely automatic. Nothing in consciousness so far as I can see that represents the judgment or the judging. The mere longer duration of the second tone, without any consciousness that it was longer, seemed to touch off the reaction."

Gou reports continually: "immediately when the tone had ceased the judgment 'equal' appeared"; "immediate judgment carried by kinaesthesis in the throat"; "immediately at the end of the perception of the second member came an attitude meaning 'shorter' and I then reacted." When special instructions were given to Gou to "institute a direct comparison and not rely on immediate judgments," in an attempt to discover whether under such instructions her method would be similar to that of the other observers, she seems merely to add to her former method by supplementing the immediate judgment by a representation of each member, which, in turn, is followed by what she calls a second "immediate" judgment.

Apart from the introspections dealing directly with the process of judgment, we find many other statements which in reality bear more directly on the question of the nature of duration than those which have been quoted. Particularly to be noted are those which deal with the two dimensions of time. Titchener[32] has written: "To the author, it seems that psychological time is . . . a surface, a bidimensional manifold, and that its two dimensions are simultaneity and succession."

In the introspections of Bo, the following is found: "I should say that the thing has perhaps what one might call duration but no temporal course. It may be something like Titchener's transverse temporal dimension. I think that I should say it all came within a single conscious present"; "I want until further notice to use the words duration and durative to mean a sort of static, non-spatial extension. This is my notion of the other dimension in bidimensional time, and it is also my notion of the measure within a conscious present. It doesn't move along although it may get bigger"; "the first tone was unitary, by which I mean it was all within a single conscious present."

F says: "attention is on the sound as a whole; i. e., the sound is all one thing; even though it has a course, the sound is nevertheless all there at once"; "in some sense or other the first sound did stay in consciousness all through the experiment. I can't say that it persists as a memory after-image, can't say definitely that it didn't repeat itself. It certainly did not become longer. (Just as a visual image may stay in consciousness with its qualitative and extensive attributes unchanged, so this sound, which is primarily a durative thing, stays without change in its durativeness?)"; "all I hear while the first stimulus is going on is one thing; in spite of the fact that the tone goes on and changes, it still is there simultaneously in some sense"; "the whole member is usually in one conscious present."

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[32] E. B. Titchener, *A Text-book of Psychology*, 1910, 340.

Gou gives a similar notion of the second aspect of time when she says "there seemed to be a holding over of the accompaniments of the auditory sensations, which may have been a sort of echo of the tone, or sometimes, a prolonging of the tone; this prolongation does not mean that the duration of the previous tone is made any longer, but just that I have hold of it, as it were."

The 'longitudinal' dimension of time is most clearly stated by F who repeatedly says that the tone ''goes forward," "has a course"; "I do not mean that the sound moves in space when I say it goes forward; I am quite sure also that my eyes do not move forward from my ear as the tone goes on. The going forward of it is, to express it in another way, an addition to the tone in a special way. It is only a little thing when it first comes and it keeps getting bigger, there gets more of it, an increase in the tone in a special way. I am not able to say in what way, save by saying that it gets longer in time. It is almost as if you added more quality of the same sort to it."

Other evidence for the progression of the tones is found in the statement that the members sometimes fall into parts. Bo; "the members generally split up into two or three or possibly more parts." D; "I sort of divided the first member up," "the tone seemed pulsy." F; "sound seems to change in quality or intensity as it goes on"; "sound seems to go forward with certain qualitative changes upon it"; "sometimes there are two or three of these changes in the tone."

The fact that three of our observers report that the members did not remain the same in intensity or quality throughout their length, and that they nevertheless report the sound as being "all there at once" and as "having duration," suggests Meumann's distinction between succession and duration (as already quoted, p. 3) and Moede's distinction (p. 4) between duration and progression.

There seems to be agreement among the observers that the consciousness of time may be taken in either of two ways. These two ways are designated by the majority of the observers as "dimensions." One and the same experience may be taken either as a moving duration, progression, or as a static duration which we shall call "length." The word "progression" seems better suited to the non-static view than does the more commonly used term "succession," because the latter name seems to imply too definite an interruption in the experience

Summary: In these introspections we find a confirmation of Katz' statement[33] that the "observers at first found that slight movements helped the judgment, but that soon these voluntary movements dropped out." We find the chest strains of Münsterberg, the expectation and surprise of Schumann, and in the "automatic" judgments we seem to have the "mechanical" judgments of Alvord and Searle. There is also a tendency toward "immediate" judgments, although there is little evidence that they are based merely on the duration of the tone. Of all the ways in which the tones may be imaged or represented, kinaesthesis is by far the most important for all observers, and in the case of one observer at least, Gou, kinaesthesis could not be eliminated. As we have already suggested, the reports contradict Nichols' findings in regard to the temporal length of representations of duration. We find, moreover, distinctions drawn between the two dimensions of psychological time, and introspections which are contrary to the statement that the subjective present does not possess subjective duration.[34]

Group 2

This group of experiments consists of a number of quantitative and introspective series in which a continuous sound (that given in Group 1) was to be compared with an "empty" interval marked off by two clicks.[35] The continuous sound, or the "filled" member, was always given first. In order that the results might be more or less comparable with those of Meumann, standard times of 60° (1.2 sec.) and 90° (1.8 sec.), with an interval of 2 sec. between members, were used. A slight change in instructions was introduced as the result of a procedure which most of the observers had adopted in Group 1. In regard to the doubtful judgments, the instructions now read: "If, at any time it is impossible to give anything but a doubtful judgment, ring in both possibilities, e. g., 'greater or equal.'"[36]

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[33] *Op. cit.*, 321.

[34] Washburn, *loc cit.*

[35] As many investigators have already noted, there is no really "empty" interval when the time between two limiting stimuli is taken as stimulus; it is merely an interval whose *filling* is different from that of the "filled" intervals. In this paper, for the sake of convenience, the times between two limiting stimuli will be called "empty" intervals. By a simple shift of connections on the apparatus (see Wundt, *Physiol. Psych.*, III, 1911, p. 344) either member could be made to consist of continuous sound or of two clicks.

[36] In the mathematical calculations judgments of "greater or equal" are considered as "greater" judgments, those of "less or equal" as "less."

Observers: In addition to Bo, D, and F, who served in Group 1, two new observers were added. These were: Mr. H. G. Bishop (Bi), assistant in psychology, an experienced observer; and Dr. H. P. Weld (W), assistant professor in psychology, a highly trained observer. These 5 observers served throughout the remainder of the investigation.

TABLE 1

AUDITORY STIMULI

Filled—empty. Filled always the standard. Five cases each.

	S = 60°				S = 90°			
	Dl_l	h	Dl_u	h	Dl_l	h	Dl_u	h
Bi............	-10.8°	.056	31.9°	.057	-5.6°	.033	23.8°	.037
Bo............	-15.4	.518	36.5	.023	-6.9	.029	21.7	.025
D.............	-1.1	.033	22.6	.032	8.1	.051	12.0	.065
F.............	2.1	.025	16.5	.058	-9.5	.033	17.6	.028
W............	-8.2	.034	20.5	.050	9.7	.017	8.8	.041

Table 1 exhibits the results of the series, the values of the Dl being expressed in degrees (50°=1 sec.). The general conclusion from the quantitative results is that: *when a filled time is followed by an empty time, the empty time is usually underestimated.* This result agrees in general with the results of earlier investigators, but no detailed comparison can be made. As has been said already, we have so few cases that our numerical results are insignificant, and the differences between our stimuli and instructions and those used by others render any explicit comparison of the figures meaningless.

Introspective results: With a change of conditions, the comparing of an empty time with a filled instead of the comparing of two filled, we find, as we might expect, at first a constant use of all kinds of aid for the judgment. As was the case in the preceding group, these aids and secondary criteria tended to drop away after a time.

Bi uses all sorts of criteria. He says: "the two members were represented in visual terms after it was all over"; "after the clicks were over, both members were run through again. This consisted in kinaesthesis in the throat"; "the length of the tone seemed to be equal to, and to be measured by, the length of a vaguely defined thing which appeared to pass before the eyes"; "tone was represented visually and auditorily"; "the tone stood out as a luminous line all there at once, while the clicks were represented by two dark points"; "duration of the tone was carried in eye-, ear-, and throat-kinaesthesis, with also certain modifications of breathing."

Bo at first relies on visual imagery, but later turns to a kinaesthetic laying-off of the first member on the second. "First member was visualised quite passively. After the second click the two didn't come up in imagery together as before, so I voluntarily imaged auditorily and visually the first click and the beginning of the tone, and ran the time through to see which would come out first. There were organics in this judgment. The end of this course was not definite. The breathing, I think, marked the end, but I didn't know of which member it was the end. Then I got the two together in visual imagery, the lengths representing duration, and compared them." When Bo was asked whether he could make the judgment without visualising the members, he replied, "being told to judge these without visualising, is like being told to add two numbers without thinking of them." Later, however, he found that he could use kinaesthesis to carry the first member over to the second, instead of calling up two visual images after the whole experience. "I should say that my attitude toward the second member, which made me judge it as shorter, was somehow conscious as the kinaesthetic hang-over from the first member"; "along with the second member I represented the first member by a course of shoulder kinaesthesis. The second click came as a surprise. The shoulder kinaesthesis was still in full swing and I should say my judgment formed immediately"; "I am quite sure, when I get the first member represented in kinaesthesis stopping before or after the second click, that I can make a judgment, although I don't always know what the judgment should be until I have a visual image."

D seems to use a method similar to this latter one of Bo's throughout the group of experiments. "I have a muscular and organic feel which seems to originate in accompaniment with the first member. Then when the second member starts, there is a repetition of this feel which is weaker, but so far as I can see is the same otherwise. The judgment depends on the effect the second member has on this feel; i. e., when they are the same there comes a comparatively gradual relaxation from this muscular tension and organic excitement. The feel, of course, has a temporal course dependent on the length of time of the first member, it runs the same temporal course with the second member that it did with the first. When the second member stops too soon or runs beyond the temporal course of the feel, there is a change in the feel which means to me a difference in the length of time." The feel, he says, he has "characterised before as expectation of the second member." D also reports "a visual pattern of the members in three dimensions," which continues the same almost all through the series of experiments.

F considered his results physically more exact when he laid one off on the other, by means of a voluntarily recalled auditory image of the first member; "the click and an auditory image which goes through the same changes that the first member went through, together make up or *mean* a repetition of the first. I always go on with the second member as if I expected it to be equal to the first;" "there is no second member for me. The second click either breaks in upon what for me is a repetition of the first member ('shorter' judgments), coincides with the end of that repetition ('equal' judgments), or comes after the repetition is over ('longer' judgments)."

W relies somewhat on visual imagery, and more on kinaesthesis.

"Before I gave the judgment, I recalled the tone and clicks in succession auditorily and visually, and respiratorily, as though I were actually producing the tone." W does not speak definitely of "laying-off the first on the second," but he apparently does this, because he almost always reports "it is as if you expected the second click to come about the time of the end of the first sound"; "expected the second click at about the duration of the first tone, and it came earlier."

Beside these common judgments by secondary images called up after the end of the second member, or some image or representation of the first member laid along the second while in course, we find various other methods of judging. "Automatic" or "unconscious" judgments are reported at times, as they were in the first group of experiments.

Bi says: "Notice this morning a tendency for consciousness to drop out, much like the action consciousness passing from impulse to reflex"; "the click interval merely seemed longer"; "the judgment came as if from the unconscious."

D reports: "Don't think there was much of anything between the last member and ringing the bell"; "perfectly spontaneous. Just pressed the button without thinking anything consciously."

F; "second click just didn't come when I knew it should. Didn't go over the experience in image. Judgment automatic"; "after second click automatically pressed the button"; "judgment just seems to be a sort of motor judgment."

F gives two further methods by which he can judge the members, one in which he "lets the judgments take care of themselves," and another in which he takes the two members in a rhythm. "I sit back much more passively. I let the tone impress me as so-and-so long. Instead of apprehending it as a thing getting progressively longer (as I ordinarily do), I apprehend it as a unitary duration. I apprehend the second member as a unitary duration. I do not make a comparison of the two durations explicitly, but allow the judgment to take care of itself. The preparation of the fore-period and the habit of reacting seem to do all the work for me, seem to touch off the judgment reaction. Have a feeling that I am physically more accurate if I judge by the other method." This method, however, he forsook as unsatisfactory, and later tried another. "I prepared myself to perceive the whole thing in a rhythm." Such a procedure proved to be practically the equivalent of "expecting the second member to stop at a certain definite point."

W reports several times that he gave a vocal-motor judgment of the absolute length of the first member.

Three observers report a change in attitude toward the experiment as the group progressed.

Bo "took signal bell and first member passively, as a matter of course"; "maintained a careless attitude throughout." Soon after the development of this passive attitude, however, Bo adopted the method of kinaesthetic laying-off of the first member on the second; and with the adoption of the new method, his remarks on passivity cease.

F; "I believe that to-day I am taking the whole experience more passively"; "none of the experience is as clear as it used to be when I was actively attentive."

W; "general feeling of familiarity about the situation, sitting back, relaxed, less strain"; "am taking the whole thing more passively. The sound went on and, my attitude being passive, I let it go on without any effort to estimate its length or anything of the sort. Have a sort of feeling that the whole thing is becoming more mechanical, somehow as though I were willing to give judgments without much pains."

Unless observers are questioned in regard to the interval between the members, they tend to omit all mention of it.

When Bi does speak of the interval, he usually reports it as "a blank," though occasionally he says "throat kinaesthesis repeating the tone"; "noticed that I inspired in the interval."

Bo reports occasional images and representations of the first member at this time.

D hardly ever mentions the interval, and when questioned says he has forgotten all about it.

F says: "the interval is just the same as if it weren't there," although occasionally he reports: "expectantly set myself for the first click."

W; "it's funny how little knowledge one has retrospectively of the interval; you know there is a gap there, but as to the real length of the gap you haven't much idea."

At times the observers also reported on comparisons which they were not instructed to report upon, as, for example, the comparative lengths of the first members of succeeding pairs of stimuli.

Bo: "There is a tendency always to judge not only between the tone and clicks presented in any pair, but also between them and those in the preceding pair. This time the tone was shorter than the preceding tone, and the clicks longer than the previous clicks. These judgments force themselves into consciousness along with the judgment I'm supposed to make. They come perfectly spontaneously."

D: "There seemed to be quite a difference between this first member and the first members of the non-introspective series to-day."

W: "There seems to be a sort of comparison of the length of the first member with the 'average length of the first members.'"

As was the case with Group 1, we find in this group a number of statements concerning the durational aspect of the experiences.

Bi: "there is not much to be said about the tone. It merely seemed to exist for a certain time."

Bo: "I doubt very much whether one ever recalls duration as duration. In recalling an interval a great part of the durativeness is the acompanying kinaesthetic course."

D: "The tone seems to be a continuous thing; when you get to the end, you still have the first part. The second member (empty), however, seems disjointed"; "when I leave the muscular feel out of consideration or try to prevent it, the second member becomes just two disconnected sounds without relation to each other or to the rest. The first member is a compact whole and has in itself duration, i. e., a continued stimulation, so that I hear the sound right along, the beginning seems to carry over and stay in consciousness until the end comes; whereas the second member seems like two disjointed points and the interval between them has no quality of duration in itself except as I put it in by means of organic and muscular sensations. The only temporal thing about the second member seems to be that the two sounds are not together."

F: "In spite of its qualitative and intensive changes, the first member appeals to me as one tone lasting a certain time. Its duration *is*, or has something to do *with*, or is impressed on me *by*, these changes;" with the passive attitude, "the qualitative aspect of the tone doesn't impress me. All I can say is, the tone impresses me as a duration;" "the first member is apprehended as a single thing *going forward*. I don't know what going forward means save this, that the beginning of the tone is in some sense still in consciousness when the last of it comes. In spite of its being successive, the tone is in a sense simultaneous."

W: "Am wondering whether both members are not altogether in consciousness, i. e., in the conscious present."

Summary of introspections on Group 2: With new conditions, secondary criteria are again prominent at first. Although these tend to become less prominent or to drop out, the tendency is, on the whole, less marked than it was in Group 1. It would seem probable, with the increase in practice, that the mechanising of the judgment which was so clearly marked with some observers in the first group, would here become still more prominent. On the contrary, many secondary criteria are reported near the end of the group. The continued use of secondary criteria may, then, be due to the difference in the stimuli in the two groups. The difference in the case of judgment of these groups comes out more significantly in the remarks made in connection with Group 6 and will be discussed in detail later. Moreover, in this group we find distinctions drawn (especially by F) between various attitudes which may be adopted toward the experiment. The "passive" attitude which he takes at times led later to a change of instructions to the observers (see Group 6). The "taking the whole thing as a rhythm" which he reports seems really to amount to an expectation of the end of the second member at a certain time, and under the latter terminology will be found in much of the later work of all observers. The fact that, unless the observers were instructed particularly to report on the interval between the members, they usually neglected to mention it or spoke of it as a blank, brings out rather clearly the fact that an observer is likely to report what he is set for (either by the experimenter or by himself), and that one reason why so many secondary criteria appear in the introspections may be due to the fact that the observers were instructed to "be on the look-out for secondary criteria." Further reference will be made to this influence of *Aufgabe* on the report under the experiments (Groups 6 ff.) which were made under other instructions. Finally we continue to find introspective evidence as to the ultimate nature of duration and its two 'dimensions.'

Group 3

This group of experiments is like Group 2 in having both quantitative and introspective series, in using both filled and empty times, in the standard times used (60° = 1.2 sec.; 90° = 1.8 sec.), and in the instructions given to the observers. It differs from the second group in having the empty time given before the filled, and in having each day the introspective taken before the quantitative series.

TABLE 2

Auditory stimuli. Empty—filled. Introspective series before the quantitative. Filled time the standard. Five cases each.

	S = 60°				S = 90°			
	Dl_l	h	Dl_u	h	Dl_l	h	Dl_u	h
Bi	18.3	.012	27.7	.022	-8.8	.016	29.5	.021
Bo	13.4	.054	-1.1	.018	10.0	.032	3.0	.023
D	17.3	.077	6.4	.028	33.2	.057	18.2	.028
F	-5.0	.044	9.7	.033	-10.1	.460	28.2	.022
W	16.1	.055	-19.4	.018	30.0	.011	-1.4	.015

The most noticeable thing about the quantitative results is the *great variation in the limens.* F shows the same tendency that he showed in Group 2, to overestimate the empty interval; W and Bo, on the contrary, tend rather to overestimate the filled interval. The introspections may throw some light on the variations.

Introspective results: The introspective reports for this group give comparatively little that has not been at least hinted in the preceding groups.

Bi still clings to visual images, giving the temporal relations of the members as spatial relations. These representations usually arise during the course of the members, and are held together for comparison.

Bo continues to parallel the second member with an "organic pattern (respiratory and neck-kinaesthesis)". In the interval between the members Bo often reports images of the clicks, and distinguishes between images of the continuous tone and those of the empty interval. The latter differ from the former; "a reproduction of the first member, I think entirely auditory. It baffles me completely to say just how it is that I get those two clicks after one another. There doesn't seem to be any lapse of time in their succession. I think I mean, however, by lapse of time, the organic accompaniments. In other words, the successive separateness of the imagined clicks is so unlike duration, as I generally mean duration by means of organic processes, that it scarcely seems as if it should be reported as such;" "the clicks came up in auditory imagery, visually supplemented, and as separated without a definite durational displacement."

D has difficulty, not only in imaging the clicks at their proper distance apart, but also with the first member while it is going on. "The time between the clicks as far as I can see now is practically empty and, as I remember, there is nothing temporal about it, i. e., I have no definite idea of how far apart the clicks were in time except the general notion that they weren't very near together, because by the time the second came, I had completely lost the first;" "the judgment is not spontaneous. It isn't self-evident which one of the members is longer and I usually arrive at my judgment by a process of elimination. I usually remember fairly well what the length of the second member is, and make a guess at the first. I can usually tell when the first member is very short or very long, but, of course, these don't happen very often. When it is in the middle ground, I think to some extent I neglect the particular member, and compare the second with what would be a 'medium length' member." Later, D voluntarily sets up a course of organic and muscular feels along with the first member, in order to have something to carry over to compare with the second. "I am compelling myself to carry the first over (organic and muscular) to compare with the second."

F continues one of his old methods, that of apprehending the second member as a repetition of the first. "The second member is perceived in the sense of a repetition of the first. I am uncertain whether I hear two clicks, image two clicks, or whether I merely *mean* two clicks in the case of the second member. I incline toward the second possibility. I do hear the tone;" "the judgment is quite an automatic thing. Half a second after I have pushed the button I couldn't tell how many times I pushed it. I certainly push it before I even have time to go over the thing in imagery."

W at first relies to a great extent on secondary criteria of several kinds, but reports with more and more frequency as the experiments continue many judgments of absolute length; "some slight strain before the second click made the first member seem absolutely long, without seeming to compare it with anything"; "after the clicks came I said 'short'"; "when the tone was ended I said 'short,' and rang the judgment 'shorter.'" W also reports occasionally as follows: "One is strongly tempted in this case to say that one interval is longer than the other and that's all there is to it. They're so short that the usual criteria are apparently, as far as I can discover, not present. The visualisation is fully as immediate a thing as one can imagine. There is no effort to visualise, it is simply there. The interval between the clicks is so long or so short, simply *there*, and the line representing the tone is so long or so short, simply *there* again, and in this case, at least, there is no attempt to measure one beside the other, the judgment is immediate; you *know* that one is longer, that is all I can say."

Some observers give evidence that the length of the members varies with the manner in which it is represented.

Bi: "as I think of the clicks now in visual terms the distance between them is very great, but in some other terms it was considerably less. I can't tell what these other terms are. They seemed to be a vague bodily kinaesthesis localised more in the head and eyes than anywhere else. The difference between these two values of click interval may possibly be the difference between the interval taken as filled time and the interval taken as empty time. The click interval seems to have empty time value while it is going on, but when the tone comes, the click interval seems to be filled up with a tone;" "this time the long value for the click interval didn't make its appearance"; "could judge the click interval either as immensely long or as awfully short"; "some trouble with the judgment because the clicks were in kinaesthesis, and the tone in vision."

Bo reports: "The members kinaesthetically together, but immediately afterward I got a visual image of the first member ending first"; "second member and represented first member ended coterminally. Immediate visual image in which the second member was seen as shorter than the first member, and a sort of organic tag to the visual imagery, seemed to verify this decision."

Summary of introspections for Group 3: The chief points here are that it seems much more difficult to "carry over" a first member when it is empty than when it is filled, and that all kinds of evaluations of the empty interval seem less certain and less spontaneous than evaluations of the filled interval. An important point for the explanation of the quantitative results is that the empty interval seems to vary in length according to the way it is represented. It is impossible from these results to say which representations seem to be the shorter. Another possible explanation for the irregularity of the quantitative results lay in a suggestion made by F at the end of the group. This observer had objected more or less all through the group to the plan of having the introspective series each day before the quantitative series, as he got into the introspective mood and it was difficult or impossible to get back to a quantitative set. It seemed that the position of the introspective series might have affected the different observers in different ways, and Group 4 of the experiments was made to test this point.

Group 4

This group consists of quantitative experiments similar to those of Group 3, except that no introspective series were given at the beginning of the hour. Some of the observers had noted during the last group that certain of their 'equal' judgments were judgments of equality,[37] while others were simply 'doubtful' judgments. In this group all observers were requested to report judgments of true equality through the speaking tube, while the merely 'doubtful' judgments were rung in as all 'equal' ones had been formerly. In the construction of the quantitative table given below, all "equal" judgments are considered together. In this group the experimenter timed the interval between the end of the second member and the ringing of the judgment,[38] although the observers did not know this was being done.

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[37] Bo reports: "The judgment 'equal' really meant uncertainty as to 'longer' or 'shorter,' a frequent meaning of 'equal' for me."

[38] By starting a stop watch when the last contact was moved and stopping it when the first ring came.

TABLE 3

Auditory stimuli. Empty—filled. Filled the standard. Ten cases each. No introspective series. Distinctions drawn between "positive" equality and "equal doubtful" judgments.

	S=60°				S=90°			
	Dl_l	h	Dl_u	h	Dl_l	h	Dl_u	h
Bi	9.3	.073	17.6	.031	7.0	.025	27.8	.187
Bo	22.8	.036	−1.9	.042	21.8	.025	20.3	.032
D	24.1	.092	−4.6	.070	37.2	.023	− .6	.025
F	−1.1	.054	17.8	.040	−5.4	.023	33.6	.030
W	26.1	.093	−20.0	.043	16.8	.025	4.1	.046

If we compare this table with Table 2, we find no apparent reason for supposing that the position of the introspective series was significant in the quantitative results. On the contrary, Table 3 shows for over half the observers a wider range between the two limens (this may be due to an increased emphasis on the possibility of "equal" judgments given by the changed instructions in regard to the equal judgments); shows no marked increase in the size of the h; shows negative limens for the same observers (except the one slight change from negative to positive in the case of W); and even shows two new negative limens in the case of D. We must conclude, therefore, that the wide individual variations in Table 2 are not the direct result of the position of the introspective series, but are probably referable to some peculiarity of carrying over an "empty" time to be compared with a filled. It will be remembered that the different observers used a variety of methods, differing from observer to observer and for one observer from day to day; and it seems natural that such a range of possible representations of the first member should result in widely divergent results.[39]

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[39] Katz (*op. cit.*, 312, 324, etc.) insists that it is possible to compare the results of different observers only when we have found the interval most advantageous for each, and when we require every observer to behave in the same way in the interval between the members; one should not be allowed to call up the first member in imagery while another does not, and so on.

TABLE 4

A comparison of "equal" judgments, divided into "positively equal" and "equal doubtful," made in Group 3 of the experiments.

	S = 60°					S = 90°				
Total number of	Bi	Bo	D	F	W	Bi	Bo	D	F	W
"Positively equal"	22	21	36	26	0	13	45	45	25	0
"Equal doubtful"	33	21	4	21	32	36	28	16	27	60

From this table it appears that the greater number of D's equality judgments are judgments of "positive equality," whereas all W's judgments are judgments of "equal doubtful." Upon reference to the introspective records, a parallel distinction between these two observers is found; D persistently relies on his muscular and organic representation of the first member as compared with the second while the second is in progress. In other groups (p. 15) he says that if this "'feel' is left unchanged by the second member, the second is judged equal to the first; if it is changed by the 'feel' of the second member, the judgment is 'longer' or 'shorter'." With such a method, the judgment would tend to be that the second was positively equal to the first, rather than doubt whether it were longer or shorter. W, on the contrary, relies to great extent on judgment of the absolute impression of the members, and so is set to get the second as "long" or "short"; and in the cases where they are near together, would naturally ring a judgment that meant he could not decide whether the second were "long" or "short." The other three observers seem to lie between the extremes of D and W in their methods of judging and would, therefore, be expected to lie between them in the kinds of equal judgments passed.

TABLE 5

Average time in seconds between the end of the second member and the beginning of the ringing of the judgment, in experiments of Group 4.

Judgments	S = 60°					S = 90°				
	<	=	>	< or =	= or >	<	=	>	< or =	= or >
Bi.........	1.2	1.8	1.1			1.1	1.4	1.3		
Bo.........	2.2	2.7	1.5	4.9	5.1	2.0	2.5	1.7	5.9	3.3
D..........	1.6	2.1	1.3			1.4	1.9	1.3		
F..........	1.2	2.0	1.3	2.8	2.9	1.2	1.6	1.4	2.2	3.2
W.........	1.8	2.9	1.1	4.5	3.5	1.4	2.5	1.2	3.0	3.2

The first conclusion from the table is that the judgments arranged in order, from that taking the longest to that taking the shortest reaction-time, stand as follows: less(?); greater(?); equal; less; greater. This is, of course, natural enough, and agrees with the introspections, which often report, when the second member is much longer than the first, that the judgment was ready as soon as the second member reached the place at which it would have stopped if it had equaled the first member; in such cases the observers often reported that they waited for the second member to finish sounding before ringing the judgment.[40]

Group 5

This group consisted of a few merely introspective series, introduced to see what the effect would be if the observer did not " carry the first over " to compare it with the second. The simplest method of assuring the desired condition was the use of simultaneous instead of successive tones. Two Stern variators set approximately for 400 and 900 v. d. and so chosen that they did not beat or make an unpleasant discord were placed at either side of the observer. The observer was told that he would hear these tones together, and that he was to judge which of the two ended last, and to give an introspection on his experience.

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[40] Katz, *op. cit.*, 447, says that the "judgment greater is always more sure than the judgment less." This agrees more or less with statements of our observers; " was certain of the judgment, though that certainty may have been merely that I reacted very quickly," etc. We should therefore expect the more certain judgments to show the shorter reaction times.

The introspective results proved to add nothing to our data, but they are quoted here for the sake of completeness.

Bi: "Judgment depends upon the fact that the sensation in the left ear lasted longer than the one in the right ear. With the judgment I nodded my head toward the left."

Bo: "Left stopped (I suppose a visual image meant the stop); and at once very smoothly, as if it was already habituated, attention (i. e., ear and eye-kinaesthesis) went to the right."

D: "the decision which is longer depends on first, the localization, whether it is sensation in the right or left ear that becomes attended to; second, on the pitch, for I know the lower pitch and bigger volume is on the left."

F: "judgment automatic, just a nod of my head in that direction"; "attention caught by the high tone sounding alone, and on that basis I judged automatically by a shift of my head."

W: "Tone to my left suddenly became clearer, involuntary movement in that direction"; "distinct sort of thing, that something was going on in the left ear when there wasn't anything in the right."

We had now completed sets of experiments with two filled, filled and empty, and empty and filled intervals. We had found many varieties of criteria for the judgment, particularly where we used both filled and empty times. The filled times were reported as "more durative" than the empty, and as it was the durativeness in which we were particularly interested, it seemed useless to complete the set of experiments by adding a group in which two empty times were compared.

It will be remembered that F reported occasionally "taking the members passively" (p. 16). Meumann had demanded a "passive" attitude with his observers; and Katz[41] replies "I can only say that when I attempted the behavior proposed by Meumann, this behavior not only caused a great deal of difficulty and a feeling of greater uncertainty with the judgment, but the judgment was not so accurate as with the behavior formerly followed," *i. e.*, when every observer followed the procedure which he deemed to procure the best results. Katz says, moreover, that "with Meumann's proposed passive behavior, the judgment is very uncertain. The sounds really come to consciousness isolated. The experience lacks the continuity of temporal course procured by strain sensations."[42] It therefore seemed advisable to carry on at least a few experiments with "passive" instructions.

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[41] *Op. cit.*, 337.

[42] *Op. cit.*, 416.

Group 6

This group consisted of introspective series in which the three previous orders of filled-filled, filled-empty, and empty-filled times were used and in which the instructions were: "As far as you can, give yourself up passively; let the members impress you passively; do not in any way 'lay off' the first on the second member." In this group only one standard, that of 60° (1.2 sec.), was used. Observer D did not serve in this group.

Introspective results: At first Bo reports the uncertainty of judgment and the isolation of the members that Katz had mentioned.

(Empty—filled) 'Determined to be passive, attention on clicks, and body-kinaesthesis and ear-kinaesthesis of attention. No reference to final judgment. Interval between members very confused, no reproduction of the first member, no specific anticipation of the second member, general anticipation involved in reference to instruction, etc. Then gave myself up to the second member. Giving-up is a sort of bodily kinaesthetic slump *plus* heightened ear-kinaesthesis which means attention *plus* the auditory processes. No explicit reference to the first member during the second, although I suppose a certain kinaesthetic uneasiness and eye-movement referred either to the first member or to the coming judgment. After the second member came a blank which was body-organics, a self-consciousness. Then I felt confused because no judgment came and I tried to get one immediately (mostly kinaesthesis). Then I reinstated the members, first one and then the other, in visual, auditory and kinaesthetic imagery. No judgment at first, the two just did not seem comparable, not greater, nor equal, nor less. Finally I got the two together visually, a concurrence which does seem to be possible in auditory-kinaesthetic reproduction. The two that stood together were the same length, but I did not have any great certainty that this visual image really represented the original." "No judgment at all after the second member. I knew that both members were rather short absolutely. Completely baffled, however, as to the judgment." "Gave myself up to the first member. After the end of it, the member was repeated in similar terms (audition and ear-kinaesthesis). The second member was clear in auditory processes, very detailed visual processes, and the kinaesthesis of auditory attention. After the second member there was no judgment at all." It will be remembered that Bo had always taken the members "very actively." He was accustomed to run the first member in organic and kinaesthetic imagery along with the second member; and, evidently, the instructions to dispense altogether with this procedure had the effect of setting him not to judge. The possibility of judging the members by any other means than by voluntarily aroused images of the two after the stimuli had ceased or by running a reproduction of the first along with the second had not occurred to him before, and so the new instructions baffled him. It is likely that in a similar way Katz' observers who were accustomed to judging by strain sensations were likewise baffled. Bo, however, in the later part of this group of experiments to a great degree stopped reporting uncertainty. He reports: (filled—empty) "The first member was clear throughout in the typically passive way. I think this typical passivity is that the tone was clear auditorily but all the kinaesthesis was irrelevant. The judgment came by way of an indefinite visual image and hand-kinaesthesis. I am not sure the visual image was essential. The feel of my hand wanting to press the button came just as soon and might have proved an adequate cue. I am rather set under this *Aufgabe* to make the judgment on any cue at all, I am so afraid of not being able to judge at all." As the experiments progressed, Bo reports judgments made on the basis of imagery arising at the end of the second member and apparently not sufficient to carry the meaning which it does seem to carry: (filled—empty) "After the second member there was a blank, almost meaningless kinaesthetic pause, a waiting. Then all at once there was an experience which *meant* 'equal.' It wasn't verbal, it wasn't numerical, i. e., it required subsequent processes for the idea of two rings to come up; it *did* mean a definite decision, and it *was*, as far as I can tell, nothing but a thing like a kinaesthetic twitch inside my head and perhaps an eye-kinaesthesis *plus* a vague background of visual imagery. How on earth such a thing gets to mean decision, not to say equality, is more than I can say." Still later, Bo gets still further away from the baffled uncertainty that the passive attitude brought at first. (Empty—filled) "Very meagre consciousness. The experience is like that of the mechanical judgment which runs itself off without consideration." "Judgment seems to be getting automatic and to have less to do with consciousness. Feeling of certainty as to the judgment without, however, anything conscious like comparison. I cannot explain the formation of the judgment. There was no reference to the first member after it lapsed, very little to the second, no comparison or effort to compare, no simultaneous visualisation or imagining." "I am not aware that I anticipated that the ring would be 'one.' I knew it was one as soon as I had rung." (Filled—filled) "At the end of the second member there came in with no effort, I think no pause, easily, passively, the idea of 'longer.' I noted that the idea was very thin and simple, that it had no obviously explicit reference to the tones, and I wonder now if it was anything more than a feeling of certainty. Think the ringing was quite mechanical." "I think of it as a reaction, not as a judgment." We have here, then, in the case of Bo an apparent dropping out of consciousness with the passive instructions. It seems probable on this evidence that if Katz' observers had been compelled to continue the passive attitude long enough, they too would have lost the uncertainty, etc. The loss of uncertainty as to the judgment may mean that with the passive attitude there is less stress on the observer to judge as accurately as he can; but this point can be settled only by quantitative series. (See Group 7.) The question of the significance of the passive attitude for our problem will be discussed at the end of the experiments performed under this instruction. (Filled—filled) "I can judge better if I don't attend too well to the tones."

Unlike Bo, the other observers did not report themselves as ever "completely baffled," although they show in Group 7 (given under similar instructions) that the adoption of the passive attitude is confusing and often results in no judgment (see pp. 29ff).

Like Bo, all report judgments without conscious comparison. Bi (empty-filled) "heard the first click, and then later heard the second one. I may have had a slight idea of the amount of time between them, but it was certainly less definite than in other experiments. The course of the tone was quite indistinct too. I heard it, but didn't mark off the ends of it at all distinctly. The judgment came spontaneously, as nearly as I can tell, touched off by the realisation that both members had been given; i. e., just as soon as I realised that both members had been given, I seemed to have my judgment ready. Except for the fact of equality there was nothing else conscious except the vaguest sort of kinaesthesis." (Filled-filled) " I think so far as consciousness goes, passive means unattentive"; (filled-empty) " at the end I began to wonder what the judgment was, and what I must ring, and when I came to ringing, I found I had already done that."

F says: (filled-empty) " Can't be analytic and at the same time passive, somehow. All I know is that the first member seemed longer than the second." (Empty-filled) " Obeyed instructions as far as I know, and the judgment went perfectly easily, didn't lay one off on the other"; " second member did seem longer. My thumb presses the button, that's all I know." (Filled-empty) " First member struck me as being absolutely long. The second member was perceived as a rhythm. Automatic judgment. Thumb just pressed the button." (Filled-filled) " Can't find a single thing in the way of mental process or in the way of changed intensity, changed general quality, changed strain, or anything of that sort that I can tell represents the knowledge that the second has gone on as far as the first; but I do *know* somehow or other, consciously or unconsciously, *precisely when* that second has gone on as far as the first."

W seemed to have less difficulty than the other observers in adopting the passive attitude. He reports: (filled-empty) " When the clicks came, the judgment was touched off at once. There was no comparison or anything of that sort." (Filled-filled) " The tone seemed very long. So far as I know there was no secondary criterion, and yet the thing seemed to last interminably. So far as I know, there was no basis for the judgment except that the first tone was long and the second was shorter." (Filled-empty) " Am rather impressed by the tendency for secondary criteria to come in when there is doubt"; (empty-filled) " I have the general *Aufgabe* to take the thing naïvely, but the determination to report makes it a good deal more difficult to maintain this *Aufgabe*"; (filled-filled) " I have always found that the successful inhibition of secondary criteria didn't interfere one whit with my ability to judge, or with the experience of going-on-ness (or duration, if you like). I've usually found, however, that when one criterion was inhibited another was very apt to take its place, and this I now believe was due to a predisposition to report and to make an accurate report, not only as regards the mental processes, secondary criteria and everything that's there, but also as regards the judgment itself. Under the present *Aufgabe*, the feeling of necessity that I must make a very accurate report as regards the judgment is lacking, and the secondary criteria fall away, so that I seem to be left with nothing but the bare experiences of going-on-ness."

Summary of introspections: Under the passive instructions, attention " tends to be lower " (cf. pp. 31f.), there is less determination to be " analytic " and to report in detail. More discussion of the influence of these instructions will be given in the next section, in the discussion of quantitative series taken under the same instruction.

What Bo means when he says he can " judge better if he does not attend too well" needs some explanation. Starting from a definition of attention as sensory clearness, he says that, even at the times when the members are clear, he does not generally report himself as paying "good attention" unless he "reacts upon them by associative supplementation." Such attention is evidently "strained" attention or "active" attention. The disagreement among various investigators (Katz, Meumann, Nichols, Kahnt, etc.) is doubtless attributable to a difference in definition of the terms "passive" and "attention."[43] If by passive one mean 'not paying strained attention,' then since, as we have seen, our observers tend to discontinue the large strains and body-movements which at first seemed necessary to the judgment, our results may be taken to agree with Meumann; whereas, if by passive is meant something like a state of irresponsible reverie, our results (p 32) may be taken in exactly the opposite way.

Group 7

This group consists of a series of quantitative experiments with a few general introspections at the end. Three orders were used; filled-empty, empty-filled, and filled-filled, and two standard times 60° and 90°. These experiments were given with Meumann's passive instructions.

TABLE 6

Auditory stimuli. Passive attitude. Filled—filled. Second member the standard. Five cases each.

	S = 60°				S = 90°			
	Dl_l	h	Dl_u	h	Dl_l	h	Dl_u	h
Bi............	10.0°	.020	4.3°	.023	25.9	.037	-11.1°	.035
Bo............	18.3	.119	18.5	.024	18.8	.042	-25.0	.007
D............	14.1	.080	-2.4	.036	32.1	.023	1.3	.104
F............	6.2	.068	2.6	.077	9.1	.080	3.9	.070
W............	10.2	.072	-4.1	.080	16.2	.037	8.0	.015

[43] Katz reports (*op. cit.*, 321), that his observers at first found that slight movements helped the judgment, but that soon these voluntary movements dropped out, because they "*took too much attention*" (italics mine). If, then, strained attention really means increased attention to bodily strains and correspondingly decreased attention to the members to be compared, it is small wonder that some writers have taken the best condition for the comparison to be "not too high" attention.

TABLE 7

Auditory stimuli. Filled—empty. Passive attitude. Filled the standard. Five cases each.

	S = 60°				S = 90°			
	Dl_l	h	Dl_l	h	Dl_l	h	Dl_u	h
Bi............	–33.6	.064	60.4	.026	–20.6	.013	43.6	.059
Bo............	–18.7	.019	66.5	.013	–23.0	.020	48.8	.026
D.............	3.8	.025	29.1	.022	19.5	.002	45.6	.016
F.............	–27.9	.031	32.4	.038	–22.2	.032	28.4	.050
W............	–15.1	.035	30.6	.030	–4.1	.022	36.9	.021

TABLE 8

Auditory stimuli. Empty—filled. Passive attitude. Filled the standard. Five cases each.

	S = 60°				S = 90°			
	Dl_l	h	Dl_u	h	Dl_l	h	Dl_u	h
Bi............	15.2°	.024	14.4°	.032	–9.0	.029	25.4°	.028
Bo............	12.0	.044	22.6	.012	16.5	.012	38.6	.014
D.............	18.1	.050	–9.4	.119	18.6	.022	21.5	.020
F.............	–8.6	.028	20.2	.046	–18.8	.016	41.7	.012
W............	23.0	.061	5.5	.020	–6.7	.006	35.0	.024

On account of the lack of quantitative results for Group I, we are unable to compare the results for two filled times under the different instructions. With the filled-empty order, we find much smaller limens under the original instructions than under Meumann's 'passive' instructions, and in general the values for *h* run higher with the "active instructions." The tendency in the case of the empty-filled times is not so marked, although the *h* show a decided tendency to be higher with the active attitude. These results, then, seem to uphold Katz in his statement that judgments under Meumann's instructions are less accurate than those under active instructions; and since the observers report that passivity in general means less attention, they refute those of Meumann, Nichols, and Kahnt in having the results more accurate if attention is high. However, as has been said (p. 29), these writers are doubtless using the word "attention" to mean "strained" attention.

Introspections on Group 7: Any introspections offered by the observers day by day were recorded, and at the end of the Group the experimenter asked for general introspections over the whole series.

Bi: "Passivity seems to mean that I am to listen to the members, but not to listen hard enough to call up imagery of any sort. Try just barely to hear the tone, to have nothing but the auditory sensation. All repeating of the first member is ruled out in the passive series. There are all degrees of certainty, sometimes the judgment came as automatically as could be. Any click interval is harder to judge than the tone interval. I feel more sure of my judgments with the active attitude, though there are certain ones with the passive attitude when it seems to go off well."

Bo: "Two tones are much easier than clicks and tone." "With two tones the judgments are much more immediate. Sometimes the passive attitude gets away with me and attention is poor. Then I have to compare the members in imagery and subjective certainty is much less. I think subjective certainty is much greater with the active attitude. It certainly is very low with 'clicks and tone,' although with a few stimuli of extreme length subjective certainty is always very high. Attention is certainly better with active attitude, at least on the average. It certainly fluctuates with the passive. You tend not to attend when you are passive."

D: "It is easiest to judge with two tones, is quite easy with these, in fact, and for that reason the matter of passivity has not been at all difficult with these, and I have a feeling that my judgments are far more accurate with both tones than with clicks and tone, i. e., that I can judge smaller differences. It was harder to be passive with the tone and clicks. Subjective certainty was certainly greater with the active instructions, except for the case of both tones, where it didn't seem necessary to be active. The passive attitude has very little muscular strain with it, whereas the active attitude has a rather indefinite muscular tenseness. With the passive, I have lapses of attention."

F: "I can distinguish several different attitudes which you can take toward the thing. If you give yourself up passively, you find yourself lost, no immediate judgment coming. You try to make a judgment, then that involves visualising the two members with eye-kinaesthesis helping out, and in a sense that is laying one off on the other, so you give that up. Then you put the emphasis on 'not laying one off on the other' and tell yourself to take the two members merely as two times. Then two things may happen: You may find that taking the click interval as a time means kinaesthesis and you find that you neglect the tonal aspect of the second member, don't attend to the tone, second member is tone in the background and ***kinaesthesis*** in the foreground, and that means you're laying off one kinaesthetically on the other; *or* you do as I have been doing lately, tell yourself you will *follow* both of them, glide along with both of them. Haven't analysed that entirely, but you seem to glide along with the first one so far, and you glide along with the second one so far, always somehow in comparison with the amount you have glided along with the first, so that in this case you have practically made the empty interval a thing that you follow as you follow the changing, the going-forward tone. . . . You can take the tone as a *time* between its beginning and end, in which case you don't attend to the tone as a tone, or you can follow the tone, and as a matter of fact you can take *both* these attitudes towards a single member, though not at once. The judgment is not immediate and it makes all the difference which of the two attitudes you take. There is a difference between the time between two points and a time that goes on and you follow; the time that is extended and the time that goes on. The natural time between the clicks is the first kind, and the natural and immediate time of the tone is the second. So that you really have to make one time over into the other by an attitude or a determination (like the determination not to take the second member as a tone) to get an immediate judgment." "Real difference between the two members temporally. In the first member the time of the first member somehow doesn't *move*, but rather *intensifies* or grows, as though you watched the time; in the second member, you follow the process, the time moves. . . . The time of the first (clicks) is a dead sort of thing; the time of the second is alive; the time of the first member is much more all-there-at-once than is the time of the second member." F here is apparently making the distinction which Moede[44] draws between duration and progression. We shall refer to this point later when in the conclusion we discuss the distinction between duration, temporal course, progression, etc.

F gives various other attitudes which he can take toward the comparison, such as taking the members as absolute impressions, etc. Subjective certainty is greater with the active attitude, particularly in the case of filled-empty and empty-filled. "I think secondary criteria are more apt to come in with the active attitude." "Strain sensations are, I think, more prominent with the active attitude, at least, since introspection is easier, they are found more, and the same holds true for images."

W reports: "Subjective certainty greater as a rule with the active, attention better with the active, and secondary criteria tend to come in more with the active. It is much easier to judge two tones than tones and clicks."

Beside the introspective evidence, it is interesting to note that during the course of the experiments Bi reported that he couldn't make one judgment because he was "too passive"; Bo remarked after some series with empty-filled order: "On the scale of 10, subjective certainty with these instructions is about minus 3"; in one case he said he had "no impulse to judge," and twice that he "forgot to judge." D reported twice that he had "no judgment, was too passive" and once that he "didn't make any judgment"; and F said once that he was "too passive" to judge.

Under Meumann's passive instructions, then, strained attention tends to become lower. Subjective certainty and even the determination to judge are less than under active instructions. We find, moreover, that the tendency not to report secondary criteria is so great here that it is sometimes impossible for the observer to say whether strains, weak images, and so on, are present or not. We have here good evidence that Meumann's observers, being under passive instructions, could not have found the strains, etc., even if they had been set to look for them; and as they were instructed definitely against getting them, it is no wonder that they did not. Neither is it any wonder that other investigators, working under a different *Aufgabe,* have strains, etc., continually reported. Meumann seems not to have worked with that degree of practice under active instructions in which, although the large bodily movements have disappeared, there still remains a definite kinaesthetic carrying-over of the first member. If he had, he might have found, with us, that accuracy is higher under active than under passive instructions.

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[44] *Op cit.*, 366.

Group 8

With the close of the set of experiments under the passive instructions of Meumann, we seemed to come to a natural stopping place. Nothing more was to be hoped from a continuation of work with either of the two instructions (active and passive) that had been used, and no other instructions had been suggested either by the introspections of the observers or in the literature. The stimuli used were, therefore, changed, and the temporal judgment of two continuous lights was investigated.

Apparatus for lights: The apparatus already described, which was used for the control of the length of the auditory members, was again used for the visual stimuli, with the exception that only one contact was used and that this was moved by both pairs of arms as in Group 1.

The arrangement of the stimulus itself was as follows. A ground-glass electric-light bulb was placed in a sound-proof box, behind a lens which projected through the side of the box toward the observer. Black paper was pasted over the lens, in such a way that the spot of light seen by the observer was 3 cm. in diameter. White paper was fastened on the outside of the hole through the box to make the light of a moderate, pleasant intensity, after the observer had become adapted to the darkness of the room. Between the light and the lens was a screen with a hole of the size of the opening of the lens. At either end of the screen were strips of tin at right angles to the cardboard. Two electro-magnets were so placed at the ends of the screen that, when a current was passed through one of them, the tin at that end of the cardboard was held against the magnet and the hole in the cardboard came over the opening of the lens, and the observer saw a circular spot of light; when the current was broken through that magnet and made through the other magnet, the tin on the other end of the screen was held to the second magnet, and the light was cut off. By means of a switch the experimenter could keep the screen held over to shut off the light until the kymograph had attained maximal speed; then, when the switch was turned, the light remained off except between the times when the first and second, and the third and fourth arms moved the contacts. Tests were made to discover the difference in the time for which the light was visible and the reading on the apparatus, and the error ever exceeded 2°, or .1 sec., with an m.v. of less than 2%. A pin-hole was pricked in the cardboard screen and in the paper which reduced the intensity of the light, so that when the screen shut off the light, there was left a faint glow as a fixation point.[45]

The *instructions* for this group of experiments were worded in a manner slightly different from that used with the auditory stimuli. Up to this time, the use of the word " duration " had been carefully avoided by the experimenter; but as the experiments were drawing to a close, and as, after the temporary introduction of the passive instructions in sequence to the original instructions, the results with the new stimuli would not in any case be truly comparable with the previous results, the instructions were worded to emphasize the term " duration." They were: " You will be shown two lights of the same or of different durations with an interval between. You are to judge whether the second is longer than, shorter than, or equal to the first in duration." Quantitative series were followed each day by introspective series. Only one standard, 60°, was used.

TABLE 9

Visual stimuli. Filled—filled. Ten cases. Second member the standard

	S = 60°			
	Dl_l	h	Dl_u	h
Bi	10.6°	.126	5.0°	.062
Bo	43.2	.022	−1.7	.023
D	22.4	.034	12.5	.046
F	.6	.061	7.4	.031
W	16.5	.048	−4.0	.042

The results for Bo are markedly different from those of the other observers, and show apparently a great time-error, for the first member is greatly overestimated (or the second greatly underestimated). The explanations are to be sought in the introspections. A similar but smaller time-error shows in the cases of all but F. Bo and D have the greatest "equality zone," F the least, and Bi and W stand between. Bo's *h* is also small.

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[45] An attempt was made at first to use the apparatus described by Moede, *op. cit.*, p. 345, but it proved impossible to get a light from a Geissler tube which did not flicker.

Introspective results: We find again gradual dropping-out of consciousness; and we seem to find some effect of the previous passive instructions.

All but one observer note that the light seems to radiate out from the spot. Bi: "The light seems to radiate from the spot during the stimulation." Bo: "The first member got very rapidly brighter until it was dazzling and streaming." D: "The light didn't come instantaneously, but it seemed to open up from around the fixation-point." F: "The first member seems to grow from the center."

As to the mechanism of judgment; Bi: "What I particularly noticed was that about the middle of the second member I found myself taking a new grip on the bell, and actually had to stop to keep myself from ringing before the second member was over. As nearly as I can tell, I didn't realise I had made the judgment, till I found my hand starting to ring. Don't think I was conscious of the first member at all after it had stopped"; "Don't remember much of the first member; nothing but a sort of impression that it had been presented for a certain time and was now over. At a certain point (where the first member would have ended) in the second member, wrist strain appeared. The laying-off was not conscious;" "First member was so very short that it seemed a foregone conclusion that the second would be longer;" "Suddenly when the second member had been going for a short time, from somewhere came a feeling that this member was different from the first, was longer, and all the time after this was really just added on. I didn't intend at a certain time to judge the two members as equal or different, but just found the judgment breaking in on me."

Bo still clings in great part to his kinaesthetic comparisons. "After the second member, no judgment. Then a sort of muscular set to ring 'one,' together with the idea 'the second is always shorter' and the inhibition and subsequent rejection of this tentative judgment. Then a definite visual-kinaesthetic successive comparison. I imaged each and bent over to it, at least in kinaesthetic image. I think the degree of the bend was the duration. Then the idea of equality (visual and muscular) and the judgment;" "was interested most of the time in the apparent shifts of the lights and quite lost sight of my obligation to note duration. At the end of the second, however, I caught myself giving a very decided nod as if I were bending forward in time with the exposure. Then suddenly I became aware I was ringing; think the first thing I knew was the sound of the bell. Very quickly, with some kinaesthetic disturbance, I compared the two visually, i. e., a visual field with two bright figures of different extension, and concluded that the judgment should be less. Don't think the conclusion was anything but a muscular relaxation of the push-button hand after I had rung once." Although Bo gets more automatic in his judgments, he still uses visual and kinaesthetic reference often. "Whole business is quite automatic, and there's not much sense to the conscious side, i. e., meanings are not obvious in the content;" "Judgment followed the second member almost automatically. Immediately after the judgment, I thought that although the two members *felt* the same length (i. e., kinaesthetic representation), the second had looked shorter." In partial explanation for the difference between Bo's limens and those for the other observers we find at the first "I haven't yet learned to set myself for duration only. The qualitative differences are still most important." All through the series Bo reports many times "after the second member, called them both up in visual (or visual-kinaesthetic) terms"; and it is doubtless the fact that the image of the first member is less accurate than that of the second, and so tends to be represented as longer than it really seemed at the time, combined with Bo's difficulty in getting away from the set to judge quality and intensity, that gives him the marked error in his results.

D at first reports the organic feels which he had in the early experiments: "The visual sensations themselves seem to play little or no part in the judgment. I mean that the visual sensations merely set up this internal set of sensations (breathing and throat) and it is from these that I actually get my notion of the length of time." But later, "The only conscious part of the judgment is the *expression* of the judgment. The judgment itself, while I'm certain of its dependence on these organic complexes, is not made consciously." "The lights have a duration in themselves, but the time, i. e., the different time-values, seems to be more or less in me; I add it on to the sensations by means of little kinaesthetic cues in my throat accompanied by a certain kind of breathing."

F: "The judgment is usually what I should call automatic. As soon as the second member is over, without thinking of what I'm doing, I go ahead and press the button." "I don't think you consciously expect the second light to last as long as the first, i. e., there is nothing conscious that represents that expectation at all; but if that second light, as in this case, simply doesn't stay there as a light as long as the first one did, then there's a reaction of your finger on the bell at once." "What strikes me is how tremendously automatic the whole thing is, even when you are set to introspect. The judgment is so automatic, you don't think about pressing the bell, but just press it, don't say 'greater,' etc., don't think them at all. Don't think about what's going to happen before it happens."

W reports practically nothing but "immediate" judgments. "Second member touched off the vocal-motor judgment 'longer';" "No secondary criteria that I could recognise; an immediate impression of longer; don't like to say it that way because the impression becomes conscious after the judgment is touched off. You just report 'longer,' and that's all." W often reports "I am taking the experiment naively."

In the experiments with auditory stimuli the sounds were reported to change "in quality or in intensity or both" during the course of the member. With visual stimuli, we find nothing of a pulsing as with tones, but we find changes in the quality or intensity of the members. Here again, we find that it is not necessary for an experience to be unchanging in quality and intensity in order to be taken as a single experience "going forward," or as a single experience "with a definite duration," or to be reported as having "its first part there in some sense when the last of it comes."

Bi: "Noticed a spreading out of the rays of light;" "saw a little light and this extended in diameter quite rapidly and then returned to its original size;" "another case of the light swelling up to a certain maximum and then immediately shrinking back again."

Bo: "The first member came in fairly intense, then was suddenly much brighter, and then got darker;" "the member got first light gray, and then after a moment, bright yellow;" "first member got very much more intense, then decreased in intensity slightly."

D: "The light seems not to be there all of a sudden, but seems to open out from the little spot;" "both the members seemed to come on and go off slowly;" "difficult to describe how the light comes on and goes off. It seems to open out toward me."

F: "The members seem to grow from the center;" "there is a coming on, a spreading of the light, and then it doesn't stay quite the same, in quality I think, mostly;" "the disappearance phase seems to take a shorter time than the appearance phase."

W: "The light got more intensive as it continued;" "slight qualitative or intensive differences in the first light, it wasn't uniform throughout the exposure."

With this group of experiments, then, we have a decided lessening of the conscious processes. This decrease began with the very first group of experiments (which used two filled times under "active" instructions), was retarded or stopped with the comparison of empty and filled intervals under the first instructions, increased rapidly again under the instructions to be passive, and continues to increase (with the possible exception of Bo) with the use of visual stimuli. This does not mean, however, as it might seem to mean, that the mere "going-on" of the members is sufficient to give a judgment of the relative lengths of the members. On the contrary, many reports are found which indicate that, although there is a going-on to the tone itself, there is no measure of the amount of that going-on until other processes (usually kinaesthesis) are introduced. That the judgment may be made without the consciousness of these processes may mean merely that the nervous system has acquired a set for a certain reaction to a certain stimulus, or that the observer is not set for introspection, or to watch for such processes. The importance of kinaesthesis in the reports of the observers was so evident that it seemed profitable to take a series of experiments in which a sense-organ was stimulated which had no primary kinaesthetic sensations of its own (as the eye has eye-kinaesthesis, etc.), and the following experiments were therefore made.

Group 9

In this group of experiments the stimuli were continuous weak electric shocks (80-83 interruptions of the primary circuit per second) upon the arm. The same apparatus for regulating the length of the members was used as formerly; but a small induction coil connected with one cell was introduced into the circuit in the experimenter's room, while the observer had a large electrode on his neck, and a small electrode (consisting of the end of a screw filed smooth and even with a strip of wood through which it was screwed, so that the observer could not tell any difference in pressure between the wood and the electrode) fastened on his arm. It was found necessary to spend about ten minutes at the beginning of the hour in preliminary work, to assure a "pins and needles" experience without any muscular contraction. The observer shifted the electrode on his arm till he found a position which gave no muscular contraction, and the experimenter varied the strength of the current until the observer reported "pins and needles."[46]

The instructions to the observers were similar to those in the preceding group. Only one standard (60°) was used.

TABLE 10

Electrical stimuli. Filled—filled. Ten cases. Second member the standard.

	S = 60°			
	Dl_l	h	Dl_u	h
Bi	9.8	.056	6.1	.069
Bo	16.5	.068	2.1	.043
D	23.0	.057	11.9	.052
F	10.1	.064	.4	.053
W	11.3	.052	.5	.042

This table shows a general agreement among the observers, although D has by far the largest equality-zone. The results show the same time-error that was evident in the work with visual stimuli and that, in fact, has appeared in all the work. It is, however, not safe to attempt any comparison between this table and any of the preceding tables except Table 9, on account of the change of instructions; and even a comparison of Tables 9 and 10 may be misleading, unless it be remembered that all the observers had had more practice in the experiment when they came to Group 9.

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[46] It proved necessary each day to make the current comparatively strong at first, because at the beginning of the hour all observers were unable to feel a current which later was so strong as to be excessively unpleasant. When the observer had once felt the current (the first time it was felt it was usually felt as strong), the experimenter reduced the current to a very small amount and, after the observer had reported "pins and needles," the quantitative series were begun. It was occasionally necessary with observer D to reduce the current again about the middle of the hour; and although this procedure changed the conditions somewhat, it changed them less than if the observer had felt the current as steadily increasing in strength.

The introspections are very similar to the previous introspections.

Bi: "This was one of the cases where in the second member the judgment occurs at that point which marks all the time before it as equal to the first member. Don't know how this point is located, but it seems that the second member has a double function. It exists in its own right as its own time, but it seems to carry along with it the time of the first member. The first member doesn't appear consciously alongside the second member at all." "I seemed to be conscious of only the second member, yet at a certain point in the second I knew what the judgment ought to be." "Attended carefully to the first member, but when it stopped, it disappeared from consciousness, there was no memory of it. I was thinking about a change in intensity in the second member, when this thought was displaced by hearing the sound of the bell, and the feel of my hand pushing the button. I knew what I rang, i. e., after the ringing was over, and felt satisfied with it." "I am just as conscious that the pricking lasts for a certain time as I am that it is a quality of pain." "Think the other factor beside a visual image this time in making the judgment was the absence in the second member of enough stimulus at the end to make it equal to the first member. This is pretty much a feeling. I have experienced much the same sort of thing when I go to a shelf to pick out a book. I am not particularly conscious that I expect to find the book there and yet I never doubt that it is there. Suddenly, now I put up my hand to take it, I find it isn't there at all. Now I seem to have an expectation set up by the first member, just exactly as my experience has made me know that a book is in a certain place. The stopping of the second member before it is equal to the first leaves me in practically the same kind of state. I think this state involves a catching of the breath, and a certain kinaesthetic set like speaking, only nothing is said."

In the case of Bo visual images and kinaesthesis are still prominent. "Judgment very easy. I was set to get durations only, i. e., didn't notice intensive, or qualitative or extensive changes, if there were any. Visualised both members as they were progressing. In the interval I saw the first member drawn out into a length which meant duration. Don't think either member got drawn out into visual extent which meant duration during its course, but there was something which *made* the two seem to have progression and I suspect that something was eye-movement. During the second there was some sort of reference to the first member, although I can't say just what it was, whether obscure visual imagery, or an eye-strain at the point where the first would have ended if laid along the second. The second was, however, somehow long absolutely and the judgment followed without any hesitation. Besides all this there was some sort of muscular aliveness in my arm which may have merely meant the localization of the stimulus, but which I suspect of referring in some way to duration." "The second member ran along very easily. I anticipated the point at which the first would have ended, and knew as soon as the second had gone beyond what the judgment was. Don't know what this anticipation is; all I can say is that I seem to be getting ready to make the judgment at some point in the second and then that suddenly my whole attitude changes. I seem to see in imagery a point in the course of the second as a significant point. As soon as the second progresses by this point visually, my kinaesthetic attitude is one of relief and the judgment is made. The push of the button was almost automatic this time." "At the end of the second, slight hesitation, then visual comparison." "Absolutely no conscious reference to the judgment or comparison, or the necessity of getting the durational aspect clear until the end of the second. Then suddenly I'm ready to judge 'equal.' The visual imagery is *there,* but it wasn't as clear as the kinaesthetic attitude of decision, and the pushing of the button itself." "Second member was much like the first except that its end is anticipated. As the second approached the point where it should equal the first, I began to press with my finger. Feel sure I have done this before, too. I think what happens in such times is that the termination of the second touches off the ring automatically." Evidence that the variety of Bo's images really confused him in the judgment is given by one case in which the second member was really nearly twice as long as the first. Bo reports: "At the end of the second I simultaneously rang 'equal' and visualised the members as unequal, I reproduced them somehow in tactual-kinaesthetic terms which *seemed* equal, although the unequal visual images persisted."

D added little to his previous reports. "Each member is accompanied by a total feeling, muscular tenseness, general sensations of breathing, throat sensations. The first one holds over and runs along with the second, just how I'm not sure. The complex isn't identically repeated, but the general attitude that is set up carries over. Then that, of course, is affected by the complex which comes from the second member, and the resultant *means directly, is* the judgment."

F: "The judgments are not just as easy and just as immediate and automatic as they were with tones, perhaps a little less disturbing and easier than they were with lights." "I am certainly sure I don't know whether the strains and relaxations are cues to the judgment or not. Sometimes it seems to me that they are; sometimes it seems to me the course of the sensation is the cue; sometimes that little snaps of my eyes are cues (touch off the judgment); sometimes when I'm not analytic at all, I wouldn't be able to tell whether those things are there or not, or what touches it off. Surely sometimes the course may be identical and yet the judgment be 'longer' or 'shorter.' Sometimes I can't tell whether there are strains there or not, and still have a perfectly definite judgment." "I am convinced that changes in intensity and quality are not what I meant by course. What I mean by course is, I think, usually the increasing duration and the changes in the bodily attitude or feeling or the vague strain." "The members just ran their course separately, and there was an automatic judgment, an automatic pushing of the bell. Certainly there is *never* a recall of the first member in image or anything of that sort. There isn't a visual image of the members while they're there, either." "Just the two members there, one after the other, each in its wave of attention, and me in a duration-comparison attitude."

W: "The judgment is touched off immediately without any reference to imagery and without any effort to compare." "There was no attempt to compare the two, the judgment was given immediately." "The judgment this time was again a judgment of duration rather than of length of visual image, although the length was there. The clear thing was a 'going-on' of the stimulus, and the second didn't 'go on' as long as the first." "General attitude one of unconcern, yet the judgment was touched off immediately and with certainty." "Judgment again touched off, that's all I can say, but with considerable uncertainty this time, though I can't for the life of me catch anything in consciousness except the deliberation, the wait; i. e., my thumb doesn't report at once. There is no thinking, no comparison, one simply doesn't report at once." "I don't think there was anything there but the duration." "The judgment 'almost equal' was touched off vocally before I rang the bell, and yet the bell rang 'shorter' and I was satisfied with the judgment. Your finger does go ahead and press the button, and then there's very apt to be a question raised after the report has been given as to whether or not it was correct."

The sensations on the arm were reported by all observers as unsteady.

Bi: "The first member was a sort of a line of pricks, as if a sewing-machine needle running very rapidly had been run for a certain short distance along the skin;" "there was a change of intensity in the second member."

Bo: "The experience splits up into parts. I visualise it often as a sand glass and as such it is as if the sand streamed first on one place, then shifted a little, then played on another area, etc., with various intensities of blast."

F: "It feels more or less like a trickle of sensation from the skin down into the tissue;" "have a tendency to express it that the sensation seems a little wavy;" "feels like a trickle of tiny drops going down in."

D: "Sensation seems to spread out in zigzag streaks;" "first member pulsated more than the second;" "member changed in intensity."

W: "It is much as if you were tapping very rapidly on my arm with a sharp pointed bristled brush."

These introspections did not bring out, as we hoped they would, the differentiation between a sense-organ which has its own kinaesthesis running along with its sensation and one that has not. One reason is evidently that the effect of practice in automatising the judgment, till it is scarcely more than a reaction, was more effective than the quality and the kinaesthetic accompaniments of the sensation. The observers seem to be arranged in a straight line as regards the number of mechanical or automatic judgments, ranging from W who gives these judgments almost without exception, through F, Bi, D, to Bo who reports them only a very few times.

Conclusion

In the course of our experiments we have found, in some guise or other, all of the secondary criteria of temporal judgments that have been reported by earlier investigators. We have found, for example, the breathing strains of Münsterberg,[47] the expectation and surprise of Schumann,[48] the bodily movements,[49] vague kinaesthesis,[50] visual imagery[51] and so on, of the other experimenters. No one of these aids to the judgment has been found to be essential; for any one could be replaced by any other, although vague kinaesthesis[52] is almost always reported, and visual imagery is the aid to which all observers turn when in doubt. We have found, moreover, that there are times when the observers are unable to discover anything in consciousness as the basis of the judgment save the mere going-on of the members.[53] Such cases were particularly frequent when Meumann's instructions were given, but they were also found before the observers had received those instructions. The first instance is doubtless an example of automatization of the reaction consciousness. The second may be due to the Aufgabe concerned.[54] As we have said, the ability to discover secondary processes depends largely on the determination given by the instructions, and Meumann's instructions, which set the observer against a detailed analysis of consciousness, could not be expected to bring out reports of many secondary processes.

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[47] Pp. 15, 39, etc.

[48] Pp. 10, 15, 20, etc.

[49] Pp. 8, 11, etc.

[50] Pp. 8ff, 20, etc.

[51] Pp. 9, 15, 19, etc.

From the results concerning the mechanism of the temporal judgment, we may conclude that, at first, all observers voluntarily institute movements of the body (of the trunk, or head, or arm, etc.).[55] After a small amount of practice, these gross movements disappear;[56] and then all observers report slight, vague strains, usually localised in the organ stimulated, although sometimes making up a general bodily kinaesthesis.[57] Visual imagery is used as a reference in most cases of doubt.[58] Finally, with a large amount of practice, all observers tends toward (and some observers reach) an automatic, immediate judgment with no conscious basis save the bare sensation, itself.[59]

From the results concerning the character of duration, we may conclude that, under proper conditions, that is, when the observers are not under too strong a determination to make a *comparison* of times, we may approximate to a mere "going-on" of sensation which corresponds to the vague extendedness of the visual field of the closed eyes. Instances of this from the introspections of the observers may be requoted: Bi, "There is not much to be said about the tone. It merely seemed to exist for a certain time." "Don't remember much of the first member; nothing but a sort of impression that it had been presented for a certain time and was now over." "Very vague notion that the first member was rather short." Bo under passive instructions gives such reports as " No judgment at first, the two just did not seem comparable, not greater, not equal, nor less," " no judgment at all after the second member. I knew that both members were rather short absolutely. Completely baffled, however, as to the judgment." In spite of this lack of judgment, Bo constantly refers to the tones as " going on," as " having a temporal course." D, " I have no definite idea of how far apart the clicks were in time except the general notion that they weren't very near together." " The lights have a duration in themselves, but the time, i. e., the different time-values, seems to be more or less in me; I add it on to the sensation." F continually speaks of the members " going forward, going on, running a course." Like Bo he reports, " If you give yourself up passively you find yourself lost, no immediate judgment coming." W says, " Under the present *Aufgabe* [passive instructions] the feeling of necessity that I must make a very accurate report as regards the judgment is lacking, and the secondary criteria fall away, so that I seem to be left with nothing but the bare experience of going-on-ness."

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[52] Pp. 9, etc. [53] Pp. 11, 16, etc. [54] Pp. 18, 32, etc. [55] Pp. 8, 9, etc. [56] Pp. 9, 20, etc. [57] Pp. 8, 15, 16, 20, etc. [58] Pp. 9, 14f, 20, etc. [59] Pp. 11, 16, 28, etc.

We have evidence that the tones can be taken in either one of two ways. The duration may be either " static " or " moving," may be either length or progression. This conclusion agrees in the main with the distinction of Moede already quoted (p. 4) between duration and progression, and that of Meumann (p. 3) between succession and duration. The distinction becomes clearer if we refer to the statements of our observers, themselves. Bi: " When the tone had run its course, I seemed to have some sort of judgment of its length." " This time there seemed to be something passing before my eyes, much as if a fine dark thread were being drawn across the line of vision. The length of time during which this running along of the thread continued was the length of the tone. It seemed as if the tone were translated over into that visual form. I call this a line or thread because it is the best term I can think of. This thread, then, began to run before the field of vision at the beginning of the tone and continued to be brought along the whole time of the tone. The whole experience was about the same as if looking through a hole and watching a train go by. I put it this way to make it clear that the experience was not so that it could be observed *in toto* after it had passed, but had to be taken just as it came." Bo distinguishes between "temporal course" [progression] and "duration" [length], by which he means "a sort of static, non-spatial extension. It is my notion of . . . the measure within a conscious present. It doesn't move along, although it may get bigger." D: "The tone seems to be a continuous thing; when you get to the end, you still have the first part." F: "The sound is all one thing; even though it has a course the sound is nevertheless in another sense all there at once." "Instead of apprehending the tone as a thing getting progressively longer, today I apprehend it as a unitary duration [length]." "You can take the tone as a *time* between its beginning and end, . . . or you can *follow* the tone, and as a matter of fact, you can take *both* these attitudes towards a single member, though not at once. . . . There is a difference between the time between two points and the time that goes on and you follow; the time that is extended, and the time that goes on." Gou speaks of a "fixing" of the duration of the first member. "There seemed to be a prolonging of the tone; this prolongation does not mean that the duration of the tone is made any longer, but just that I have hold of it, as it were." W: "When the tone was over, there was an effort to hold it." "I think of the member as being unrolled." "The first member seems to stand there in the interval." "These intervals somehow have a sort of unity." "Have a feeling that one *perceives* a duration in the same sense that one perceives a tree. It seems to be a unitary sort of thing."

Our results also point to the conclusion that all sensations have the inherent character of going-on or of progression. In this conclusion we agree with all those psychologists who give duration as an attribute of sensation. From the experimental work we may cite as examples of this position: Mach's earlier theory that all sensations are accompanied by the time sense; Vierordt's statement that the "spatial and temporal dimensions of sensory stimuli come, so to speak, immediately into our consciousness"; Nichols' conclusion that "duration is an attribute of every sensation"; and Meumann's presupposition that "the processes of our consciousness . . . are given constantly at the same time as processes of a temporal nature." From the psychologists whose concept of duration as an attribute of sensation is based less directly upon experimental evidence, we may cite Külpe, although we can not agree to the identification of physical and psychical time, and Titchener.

We come now to the question of the place of duration as an attribute. As we have already said, there are two ways of taking the temporal experience, as progression and as length. These stand at quite different levels, and are the results of quite different attitudes toward the experience. A sensation taken as it comes immediately to one, as it comes under a merely existential determination, progresses. The determination to compare or to estimate, however, tends to result in a taking of the experience as a length. Progression is the more ingrained, the more vital aspect of the experience; without progression, length is impossible. Length is something that may or may not be added on afterward and does not belong to the sensation as such. That is, the sensation has length only in retrospect, has length only after it is over, while it has progression while it is going on. In fact, the going-on *is* the progression, and by going-on we do not mean continuance in physical time, but the immediate experience of going-on. The length is, so to say, a " fixing," a " making static " of the progression for a simultaneous view. It is a consciousness which involves supposing that the first of the tone is still there in some sense when the last of it comes. Now, to obtain this view, the progression must be referred to something outside itself, it must be given a definite beginning. As progressive alone, the experience seems to have no beginning or end. It is related no more to beginning and to end than the field of the closed eyes is related to definite points, say the furthest we can see to right and left; it is mere going-on, that is all there is to it. Length, on the contrary, is most clearly expressed as temporal *distance* between two points. It is the result of a perceptual, rather than of an attributive attitude. We find, from our introspections, that it is more difficult to harden the progression into length at some times than it is at others. At such times the observers report that the member was so long that the first of it was gone before the last came. That is, it did not " compose a single perception," did not impress them " as unitary," " was not in a single wave " or " single span of attention," " was not contained within a single conscious present." For them, as we have seen, progression goes on within such a time, whereas in general the conscious present, taken as a whole, as a perceptual unitary thing, *is* itself length.[60]

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[60] When Washburn (*loc.cit.*) maintains that the conscious present has no subjective duration, she is quite evidently thinking of duration not as progression, but as the simultaneous view of the experience which we have called length. That she can deny that *progression*

If, then we take duration in the sense of progression as an attribute (and duration in the sense of length as a percept),[61] there arises the question of the status of this progression attribute as compared with that of other attributes. It evidently does not hold the unique place of quality, but belongs with the quantitative attributes which furnish the basis for answers to the question " How much ? " Just as extendedness is the basis for answers to " How far ? " and intensiveness for " How much ? ", so progressiveness is the basis for the answers to " How long ? " Of the intensive or quantitative attributes, extent seems most closely related to progression; both are vague extensions, continua, only the one is of space, the other of time. Progression is also more or less similar to the generally admitted attribute of intensity. Indeed, our observers sometimes say " it is like having some more of the same thing added on to it." We take it that you can " add more of the same thing " in three different ways: add to it beside itself, i. e., spatially; add to it after itself, i. e., temporally; add to it at the same place and the same time, i. e., intensively. Of these three, intensity is the only one admitted by all psychologists. Its enviable position is due, not to any superiority which it has over extent and progression, but to the fact that Fechner stated its position flatly, and others agreed to his decision without requiring introspective evidence. If, instead of this introduction, intensity had received the experimental treatment that has been accorded to duration, it too would have gone through the stages of being taken as a relation, and as a result of secondary criteria and mediate judgments, through which duration has passed. But in time we should have found, as we have found for duration taken as progression, an inherent aspect of sensation which we must call an attribute.

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takes place within this present seems hardly possible. If succession be taken as equivalent to progression, her statement that the " psychologically primitive time judgment is one of succession, not of duration " seems rather to bear out our own thesis that duration, in the meaning of progression, is an attribute of sensation.

[61] Distinctions like that between length and progression do not seem to us peculiar. Quality, extent, intensity, clearness, can all be taken in a similar way. Bare " thatness " is attributive; " redness " (as distinct from blueness or orangeness) is perceptual and involves a reference to other modes of " thatness " or to points on a scale of qualities. Bare " spread-out-ness " is attributive; size is perceptual. Bare " muchness " is attributive; " so loud " is perceptual. Bare " liveliness " is attributive; " so clear " is perceptual.

ON THE PSYCHOMOTOR MECHANISMS OF TYPEWRITING

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"The record of the accuracy remains long after the speed is forgotten."

Typewriting is one of the most important industrial operations. To many persons it is the direct means of livelihood and other persons are continually undertaking its study with a view to making it such. There is perhaps no psychomotor process so directly open to experiment in which efficiency is of such wide practical value. Scientific inquiries into increasing the efficiency of typewriting on the psychological side may have a two-fold aim,

1st. To discover the symptoms which presage success or failure in acquiring the accomplishment, in order that the probable progress of the learner may be predicted, that he may be encouraged so far as possible in a career which promises good results for him, or dissuaded as soon as possible from wasting time in something in which there is practical evidence that he will not succeed.

2nd. To study the various conditions of efficiency in typewriting as affected by different times of day and different work periods, and different techniques of operation, etc., in order to increase so far as possible the efficiency of the skilled operator.

The present experiments were made to get a further insight into the means of attacking these questions and also as a practical introduction to the broad problem of studying psychomotor adaptations experimentally through the media of choice reactions.

Non-Experimental Observations.—Typewriting is in a special position in that it is possible not only to make measurements of performance in it experimentally, but also to compare these with fairly accurate observations under non-experimental conditions where the operator is unaware that the performance is being measured. Subsequent to the experiments, a few observations of this sort were undertaken in which the operators, both of whom are professionals of years' practise, of course worked at their accustomed desks, and at their own machines, which are Remingtons. They did not write the same material but it was of the same sort, namely portions of clinical records. Intrinsically, one operator's copy may have been slightly more difficult than the other's, but on the other hand this operator is much more accustomed to this sort of copy than the other. Timing was with the stop watch, by pages, and the finished product was later examined for errors. Results are shown in the following table:

FASTER AND LESS ACCURATE OPERATOR

Observation	Number of Strokes in measurement	Average Strokes per second	Errors	
			Number	Per cent
Day I—*a*	374[1]	3.17	5	1.34±
b	1518	4.34	3	0.197
c	1466	4.14	3	0.204
d	1694	4.58	7	0.414
e	1613	4.48	9	0.559
Day II—*f*	1647	4.64	4	0.243
g	1466	3.44	10	0.681
h	1583	3.91	4	0.253
i	1611	4.74	9	0.559
Average experimental performance	1581	5.27	11.9	0.753

SLOWER AND MORE ACCURATE OPERATOR

Observation	Number of Strokes in measurement	Average Strokes per second	Errors	
			Number	Per cent
Day I—*a*	3507	3.88	5	0.142
b	3620	3.81	3	0.083
c	3545	3.21	4	0.113
Day II—*d*	3206	4.24	5	0.156
e	3338	3.62	2	0.060
f	856	3.86	1	0.117
Average experimental performance	1413	4.71[2]	8.5	0.601

[1] At this point the operator removed the sheet and began the page afresh

[2] This operator shows some practise gain in the experiments, *cf* below.

In each operator, the non-experimental rate appears in general as nine-tenths of the experimental rate; but it is really somewhat closer to it than this because the non-experimental observations include stops for erasures and other pauses that are not wholly relevant to the measurement. The time for external interruptions, when they chanced to occur, was taken out. The experimental conditions cannot, according to the above, be said to have produced, as such, individual differences in speed foreign to the differences of ordinary performance. The case is quite otherwise with the errors, for while in the former operator the errors in the non-experimental observations are 55% of those under experimental conditions, in the latter operator they are but 18.6%, the latter operator making, in general, over five times as many errors with the experimental conditions as without them, the former one only about twice as many. There was a noteworthy difference in the co-operativeness of the two operators in the experiments, to which this may be related.

The Experiments.—The machine used was a Remington Standard Typewriter No. 10. Contacts with platinum facings were so adjusted to the machine that one electric circuit was made whenever a key or the spacebar was struck, and another only when the spacebar was struck. These circuits operated signal magnets whose movements were recorded by ordinary graphic methods. The speed of the recording surface was approximately 12 mm. per second, the time intervals being marked by a Jaquet Chronograph.

The table and chair employed experimentally had been used for years in typewriting by both operators, though it was not being so used by them at the time of the experiments.

The experimental work by each operator consisted in writing for five minutes at the beginning of work, about 8.30 each morning, and for five minutes again at the conclusion of the morning's work period, about 12.00 m., for fifteen days. Both operators of course wrote the same copy on corresponding days. The copy was taken from Emerson's Essays, as a text free from dialogue or italics. This material is foreign to what the operators are regularly accustomed to (clinical records and business letters), being more involved and obscure, with a different vocabulary.

The external marginal stop was set so that the bell rang at 61, and the stop became effective at 69 on the scale.

The operators were not given verbal instructions, but typewritten ones, which were as follows for the first ten days:

> At the word "Go," copy the given text, beginning at the point marked with pencil, and continuing until the word is given to stop, as it will be after five minutes of writing. The lines of your writing will be single spaced. Write at your best working speed, that is, the speed by which you would wish your efficiency as an operator to be judged. Make no stops for corrections, make no strikeovers, and do not use the back-spacer.

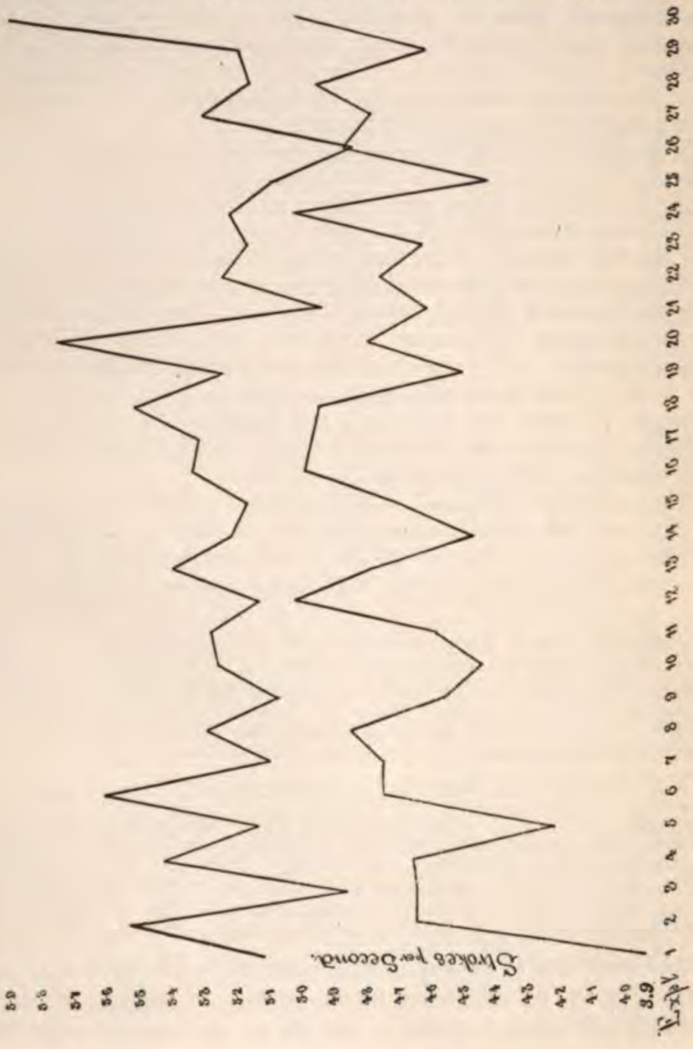
For the last five days the instructions were as follows:

> At the word "Go," copy the given text, beginning at the point marked with pencil, and continuing until the word is given to stop, as it will be after five minutes of writing. The lines of your writing will be single spaced. Aim to make no errors, and write at the best speed that will make this possible. Make no stops for corrections, but the back-spacer and strikeovers may be made use of at will.

The instructions should have been given in the reverse order from that here quoted; from this standpoint the procedure is not very sound.

The reaction of both operators to these instructions was to write at abnormally high rate for them, with a correspondingly large number of errors.

Elapsed Time of the Total Process.—This concerns the amount actually written in the 5 minutes—the ordinary practical measure of typewriting speed. It is usually stated in



terms of "words per minute," but this means very different things according to the class of copy used, quite aside from its familiarity. More precision may be sought by measuring not the number of words but the number of *strokes,* that is, touches which result in a forward movement of the carriage. The space-bar is here counted as a stroke, though it is much quicker than the strokes of the keys. On the other hand, shifted keys also count as one stroke. This number of strokes is the absolute measure of the amount of typewriting done in the period; it is considerably affected by at least one other factor, the time of returning the carriage after each line. The question of accuracy is separately dealt with. The following curves give the average number of strokes per second, for each five minute period of writing in the thirty days' experiments, with the two operators.

Time of Separate Processes; the Carriage Return.—This is the longest single process involved in typewriting. With the present machine, it requires merely a backward sweep of the hand, against the proper lever. The average time per line consumed in this process was distributed as follows for the two operators:

Seconds....	.4	.5	.6	.7	.8	.9	1.0	1.1	1.2	1.3	1.4	1.5	1.6	1.7	1.8	1.9	2.0	Over 2.0
Records:																		
1-15....	1	..	27	61	69	32	28	12	15	15	4	11	1	1	6	1	1	7
15-30....	1	2	51	70	85	37	22	21	15	9	4	4	1	..	4	4	2	8
Records:																		
1-15....	1	..	..	..	1	14	42	52	55	49	29	21	9	15	2	3	..	11
15-30....	..	..	..	..	1	7	31	49	95	50	42	14	14	5	8	5	..	1

The general median time of the carriage return in the first quoted operator is .87, in the second quoted, 1.28 seconds.

Book's allowance of three strokes for the carriage return would therefore be too little for these operators. The carriage return takes the time for about five and a half correct strokes in the first operator and for about seven and one-third correct strokes in the second.

The methods habitually used by the two operators are not the same. One operator brought the thumb against the hook in the lever, and pushes it back with a single extensor movement of the arm; the other grasped the lever with the fingers, giving it a distinct twist. These latter motions result in a decrease of some 3% in total typewriting speed, not to mention the additional muscular strain put upon the arm.

Carriage Return in Paragraphed Lines.—Although the first lever of the column selector was set at 10, and the start of every portion of copy was paragraphed to begin at this point, neither of the operators used the column selector for paragraphing, but returned the carriage by hand for the paragraphs also, making corrective movements (or not) with the space bar and back-spacer. One operator aimed to begin paragraphs at 10, but began them elsewhere, usually at 11, in 8 cases out of the total 36; the other aimed to begin paragraphs at 5, but began them elsewhere, regularly at 6, in 9 cases out of the total 30. When the next line begins a paragraph the time of the carriage return is distributed as follows in the two operators.

Seconds......	.9	1.0	1.1	1.2	1.3	1.4	1.5	1.6	1.7	1.8	1.9	2.0	Over 2.0	Median
Operators...	1	3	1	5	3	7	3	..	..	..	..	2	4	1.43
	..	3	2	4	5	1	1	3	3	2	1	1	4	1.45

Carriage Return as Affected by the Marginal Release.—One of the operators made no use of the marginal release throughout the experiments. At the end of experiment XIII the other observed that it was set before the end of the line and during the succeeding experiments operated it in all 19 times. Apparently the fact of having been preceded by an operation of the marginal release makes the carriage return slower than it would otherwise have been by over 33%, as shown in the following distribution:

Seconds	.7	.8	.9	1.0	1.1	1.2	1.3	1.4	1.5	1.6	1.7	1.8	1.9	2.0	Over 2.0	Median
Operator...	1	1	3	2	3	4	1	1	..	..	..	1	..	..	2	1.17

Time of Operating the Marginal Release.—The time of operating the marginal release in the 19 cases mentioned above is between 1 second and 2.5 seconds, averaging 1.6 with an m. v. of .36 seconds.

It is thus apparent how the use of the marginal release cuts into the typewriting speed, not only by the time taken to operate it but by reducing the speed of the carriage return, so that the learner should be firmly trained to resort to the marginal release as little as possible.

The Back-spacer; Inhibition and Time for Operating.—Under the conditions it was not practicable for the operators to overcome the long-standing habit of using the back-spacer. One operator used the back-spacer 6 times during the days it was not supposed to be used, the other 69 times. The inhibition gradually improved, the uses of the back-spacer being distributed as follows through the successive experiments.

Experiment.......	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
No· of backspaces.	10	6	3	8	4	1	6	1	5	2	1	2	4	4	4	2	1	2	2	1

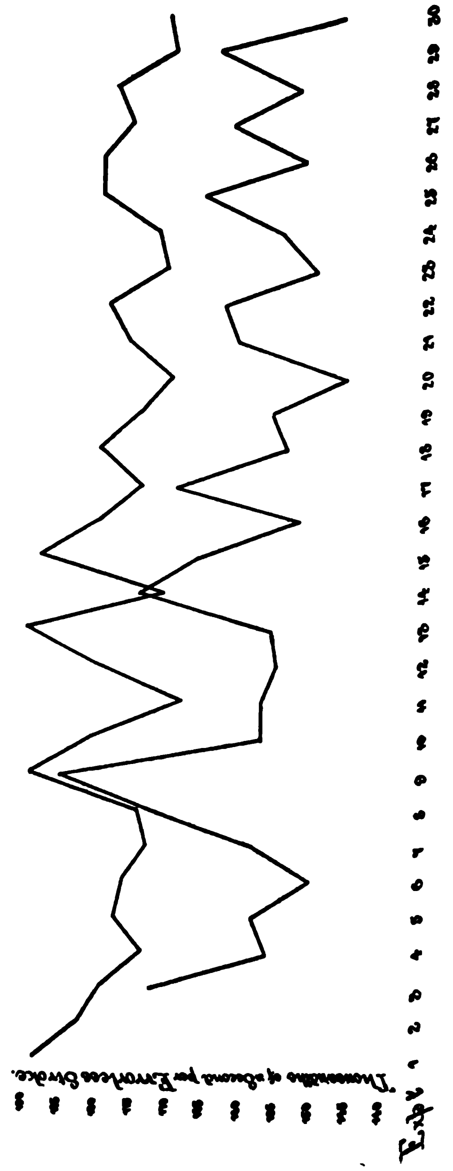
For the last 10 days the use of the back-spacer was part of the conditions of the experiment. The strikeovers were measured, giving the time lost through the necessity of operating the back-spacer. This is considerable, averaging about a second in each operator; a back-spaced strikeover thus takes as much time as five or six correct strokes.

Average Time of Strokes in Errorless Typewriting.—This comes as closely as possible to the actual speed of the typewriting process uncomplicated with extraneous factors such as the carriage return, the back-spacer, etc. As a more or less arbitrary determinant is taken the time required for three errorless lines in each record that contain the least evidences of blocking. The time for writing the lines is divided by the number of strokes in them giving the average time for each stroke. No statement of variability can be given for individual strokes. The results for the successive 30 experiments are as follows in the two operators: (See cut, p. 54.)

These figures must not be thought of as true reaction times. These it is impossible to obtain because the instant of stimulation cannot be fixed. They are shorter than most elementary choice reaction processes, indicating a large amount of overlapping, of which indeed one seems to be introspectively aware.

Steadiness of Typewriting Rate.—The method employed does not lend itself to precise measurement of the variations between the individual strokes. If the experiments were being repeated more attention would be paid to this. Instructions to learners lay some stress on the value of sacrificing speed to steadiness, and psychology has seen in the mean variation a criterion of attentional control. The difficulty in the way is that the rate in writing any material is very dependent on the familiarity of it to the subject and of course the same material is not equally familiar to different subjects any more than it is in shorthand. At the beginning of typewriting practise this difficulty would not be so serious, and a good measure of steadiness of writing rate will be an important factor in predicting the future efficiency of learners.

Book calls attention to the presence of various important processes in typewriting, of a more strictly mental nature, which do not show in the learning curves. If this means that an accurate control of introspection is necessary for the prediction of future efficiency in the learner the practical value


Exp.
Thousandths of a Second per Erroneous Stroke.

of typewriting measurements will indeed be seriously limited in this direction. But it is doubtful if the actual conditions of learning have yet been closely enough studied, with the following up of the future efficiency of such learners, to warrant pessimistic conclusions.

Relation of Speed to Accuracy.—Of course we know that in general practise improves both speed and accuracy in typewriting; the novice writes slower and makes more mistakes than the expert. Between individuals a broad positive correlation thus obtains between speed and accuracy that need not be confirmed experimentally. On the other hand, if an individual speeds above his optimal rate, more errors are produced, and the same is the case if he tries to write slower. In these experiments the slower writer also makes the fewer errors. A more legitimate question is whether, on days when a highly practised subject writes faster, he also tends to write more accurately or not. In calculating this for the present operators the first ten records were excluded on account of their showing, at least in one operator, too much indication of practise.

The measure of accuracy is here the number of *errors* made in the five minute period. The measure of speed is the average time per stroke for the five minute period.

For the last 20 days the Pearson coefficient of correlation between the given measures of speed and accuracy were .44 for one operator, and .54 for the other. This is in line with the general findings in other psychological functions; increased speed and accuracy go hand in hand, " more haste less speed." It is not conclusive however, since the material necessarily varies in difficulty from day to day and an easy passage is of course written more accurately and faster than a difficult one. Then too, errors tend to retard the rate through the blocking which the consciousness of them produces. The best approach to the matter is by comparison of the rate of false strokes with that for correct ones, and this we are not in a position to do here. Very trustworthy data of a similar nature have however been obtained by Henmon, who has kindly put the material[1] at my disposal for the purposes of this study.

In central tendency, false reactions are regularly shorter than correct ones, but the differences are not so great as to raise the question of essential prematurity in their production, save for Subject S. in the experiments on lines. Often they are very much longer and they are, as would be expected, quite variable. Since nearly every false reaction has an even chance of being superficially "correct" there are doubtless many more of them than come to light statistically. A subject may direct from his own introspection that a certain reaction be discarded even though the correct movement has been made. Though it is not by any means so regular as with the correct reactions, the false reactions show the same general tendency to become longer as the difference between the stimuli to be discriminated becomes less.

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[1] Henmon: On the Time of Perception as a Measure of Differences in Sensations. *Archives of Philosophy, Psychology and Scientific Methods,* No. 8, 1906.

Number of Errors and False Strokes.—The process of typewriting from copy involves a great number of fairly complicated psychomotor adjustments following upon each other in rapid succession. These adjustments do not always run smoothly, but on various occasions incorrect and false adjustments occur. These false adjustments result in "errors;" their effects are seen in false strokes upon the machine making imperfections of the transcript. Two points of view are possible in considering the mistakes. From the immediate objective standpoint it is the "false strokes" alone that count, the more false strokes the worse the copy, but from a psychological standpoint it is plain that circumstances may arise in which a single faulty mental reaction may result in an indefinite number of false strokes. When *admiration* is read and written *ambition* the number of false strokes is a number equal to the letters in the word wrongly written, and yet there is only one real psychomotor "error." Most errors involving more than two false strokes are of this kind, that is, due to misreading the copy. Thus,

1. A *false stroke* is any stroke followed by a forward movement of the carriage which requires correction in order to produce a perfect copy.
2. An *error* is a faulty psychomotor adjustment resulting in the occurrence of one or more false strokes.

The actual number of errors in the records together with the false strokes they involve is as follows in these experiments:

	OPERATOR						OPERATOR					
		No. of false strokes they involve						No. of false strokes they involve				
Exp.	No. of errors	1	2	3	Over 3	Total false strokes	No. of errors	1	2	3	Over 3	Total false strokes
1	20	17	2	..	(38)	59	20	17	2	1	..	24
2	13	12	1	..	..	14	15	13	2	..	..	17
3	5	4	1	..	..	6	12	12	..	..	..	12
4	11	11	..	..	..	11	10	9	1	..	..	11
5	12	9	1	1	(7)	21	8	7	1	..	..	9
6	12	8	2	1	(47)	62	14	12	1	1	..	17
7	19	16	2	1	..	23	13	11	1	..	(10)	23
8	9	8	1	..	..	10	6	6	..	..	..	6
9	10	5	2	3	..	18	17	15	2	..	..	19
10	4	4	..	..	..	4	10	10	..	..	..	10
11	5	3	1	..	(36)	41	11	8	1	1	(39)	52
12	3	2	1	..	..	4	21	19	1	1	..	24
13	8	7	1	..	..	9	12	11	1	..	..	13
14	15	13	..	2	..	19	14	13	1	..	..	15
15	5	5	..	..	..	5	14	13	1	..	..	15
16	5	5	..	..	..	5	13	10	3	..	..	16
17	4	2	1	1	..	7	13	12	1	..	..	14
18	8	7	1	..	..	9	11	6	4	..	(10)	24
19	12	8	1	..	(8), (43)	61	19	17	1	1	..	22
20	8	7	1	..	..	9	7	7	..	..	..	7
21	11	11	..	..	..	11	9	7	2	..	..	11
22	6	6	..	..	..	6	12	10	..	1	(43)	56
23	6	6	..	..	..	6	11	9	2	..	..	13
24	7	7	..	..	..	7	10	9	..	1	..	12
25	7	6	..	..	(78)	84	13	10	2	1	..	17
26	7	5	1	..	(8)	15	13	12	..	1	..	15
27	7	7	..	..	..	7	9	5	3	1	..	14
28	5	5	..	..	..	5	10	10	..	..	..	10
29	8	8	..	..	..	8	6	5	..	1	..	7
30	4	4	..	..	..	4	3	2	..	1	..	4
Av.	8.5				Total	550	11.8					509
M.V.	3.5						3.0					

By far the greatest number of errors involves only one false stroke; that is, they represent simply the striking of one wrong key, after which the writing proceeds correctly. Those involving two false strokes are nearly all transpositions. Errors of more than two false strokes, including many with excessive numbers of false strokes, are practically all misperceptions of the copy.

Kinds of Errors, Different Psychic Levels that they Represent.—An error is the product of interference with the normal adjustive processes from somewhere. The error of writing *admiration* for *ambition* is obviously a very different sort of error from that of writing *amvition* for *ambition* and one much more serious in its effects upon the copy, just as the despatch of false orders on the battlefield is more costly than the blunder of an individual soldier. Originating as they do quite outside of and not involving any defect in motor adjustments they may be thought of as errors of the highest level type.

Superficially there is a distinct line of cleavage between the uncommon, but far reaching errors of this sort, and the ordinary mistakes which are simply substitution, transposition or addition of a stroke or two. Three questions present themselves in regard to these very transitory breakups in the adjustments. First their relation to consciousness, second their relation to the time of the immediate and surrounding processes and third their relation to the content of the copy surrounding them.

On the first little is to be said here, indeed the conditions inherent in the normal typewriting process are prohibitive of immediate introspection. It is doubtful if much more can be done with this feature than Book has already done. There may or may not be consciousness of having made an error. The faulty ideomotor processes that are expressed in the error may or may not be present in consciousness before the error occurs. More regularly however it is the actual making of the false stroke that brings the process into consciousness, at least so far as insight into its falsity is concerned. Sometimes the idea of falsity (whether conscious or not we have no means of knowing) seems to come before the stroke is completed so that the key while wrongly struck leaves an impression lighter than the normal. As under the conditions of typewriting the memory of all conscious process must very quickly fade, one can seldom be even half sure of how much in any given process has been conscious.

One reaches firmer ground in the time relation of the errors. Where an error involves a block in the typewriting process at all, which it does not necessarily do, its most frequent position, in the data examined, is directly after the false stroke. Sometimes it does not come for another stroke and again it may come a stroke before. Occasionally too, there is a slowing of the writing process for some strokes before the error, indicating a gradual failure of the adjustments, though we do not know its relation to consciousness. The length of these blocks is seldom over 1½ seconds. Through the unfortunate mistake of trying to eliminate the use of the back-spacer the interpretation of these blocks is not clear. The writer believes that they represent essentially the inhibition of the tendency to use the back-spacer the instant there is awareness of a false stroke, and that the back-spacer is struck nearly, if not quite, as promptly after the false stroke as the next key would be after a correct one. The indication is certainly that false strokes sometimes occur with insight into them and sometimes not.

What lies back of the failures of attentional control that show themselves in the errors is unfortunately beyond the scope of this study. What stroke is falsely made may be governed by quite deep-lying sources, and from the immediate surroundings of the copy, and from certain motor habits formed in typewriting common words, as will be illustrated.

It should be noted that the false strokes are generally effective strokes at wrong keys; inaccurate fumbling strokes at right keys play an insignificant part. During the entire gathering of the material no instance is recalled of two keys struck simultaneously so as to lock them.

The errors fall naturally into four sorts,—omissions, substitutions, transpositions (metatheses) and additions.

Their material will be presented in this order. It is most probable however that similar psychic mechanisms may result in any of these errors, what kind of error results depending mostly on the precise point of time at which normal control over the whole process is restored.

In quoting examples of the errors, it is not practicable to reproduce their smoked paper records, but it is usually desirable to indicate some features of them diagrammatically, i. e., slowness in writing rate, the place of occurrence of a block and its length. The scheme is adopted of leading the letters in the word where the writing is obviously slow, and indicating blocks by a row of periods inserted where the blocks come on the record, and indicating at this point the length of the block, thus,

Error	Correct Form
f a m i k$\overset{1.4\text{ s}}{\ldots\ldots}$iarity	familiarity

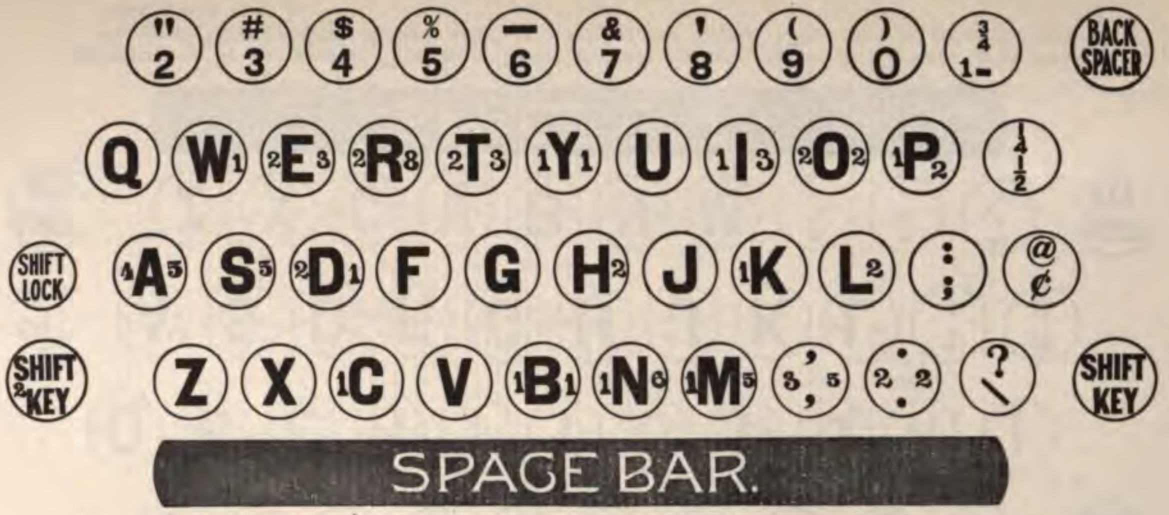
would mean that after a period of slow writing of the word up to this point, *k* was erroneously written for *l* and that immediately afterwards a block of about 1.4 seconds ensued after which the writing was again taken up normally. This scheme, supplemented by remarks, is used in representing nearly all the errors quoted.

Omissions.—An error of omission in the transcript results whenever the psychomotor process for striking the given key is not sufficiently effective to produce the legible impression of that key. A key may be struck too lightly to make a legible impression and yet strongly enough to trip the carriage. It is possible and even probable that many of these cases are abortive false strokes but there is no means of knowing this. In one of the operators there were 36 cases in which the carriage moved forward without a legible impression of the key struck. In the other the number cannot be arrived at, but the number of back-spaces observed is often greater than the number of strikeovers, which excess probably represents the correction of omissions of this kind. The cases in which the carriage did not move forward are illustrated as follows, the numerals lettered on the chart indicating the number of times each stroke of the given key was omitted by each operator.

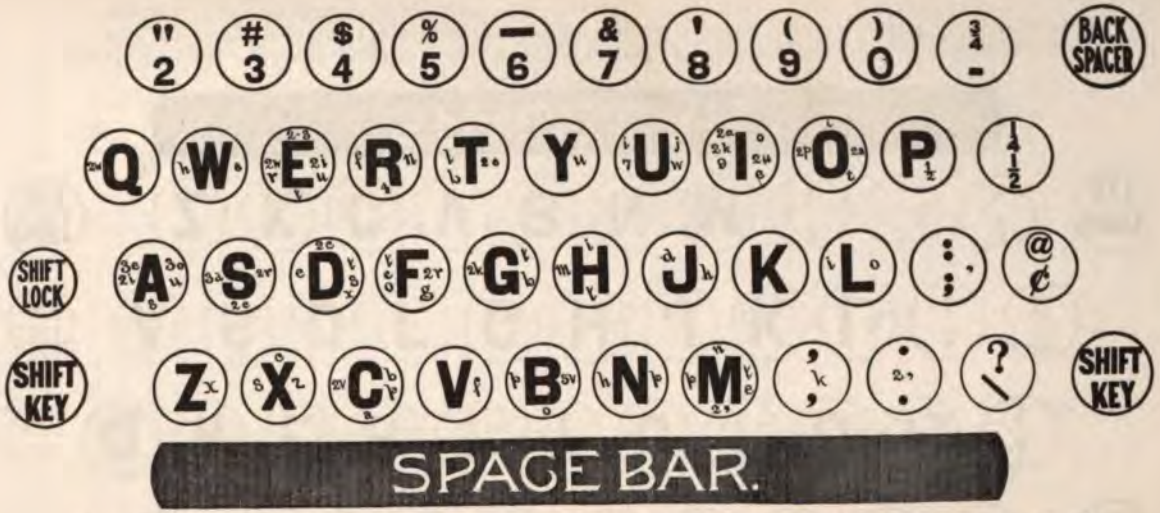
Certain keys seem to be subject to omission out of proportion to the number of times their strokes occur, but it is not possible to make further interpretation of this; *m* and *n* seem relatively difficult to reach. Operator J shows many more of these omissions than operator T does and they are chiefly distributed among the keys *a, s, r, m,* and *n.* The letter *a* is that most frequently omitted by operator T. The letter *e,* by far the most frequent of all, is not especially subject to this or any other sort of error.

Substitutions, Transpositions, etc.—The substitutions are illustrated in the accompanying chart. Here again the number of errors on a given key may be out of proportion to the frequency with which its stroke occurs. The two operators also differ in this respect. Operator T has many errors on *m* and *n.* Operator J shows distinctly more errors on *m,* though *n* is the more frequent letter. The kinds of errors also differ. Operator T shows evident cases of tending to substitute one particular stroke for another, as *j* for *h, z* for *x* and the comma for *m.* The errors of Operator J are more scattered except that *v* is often struck for *b.* In the errors made on *a* operator T substituted mostly neighboring letters. Operator J writes other vowels without regard to their proximity.

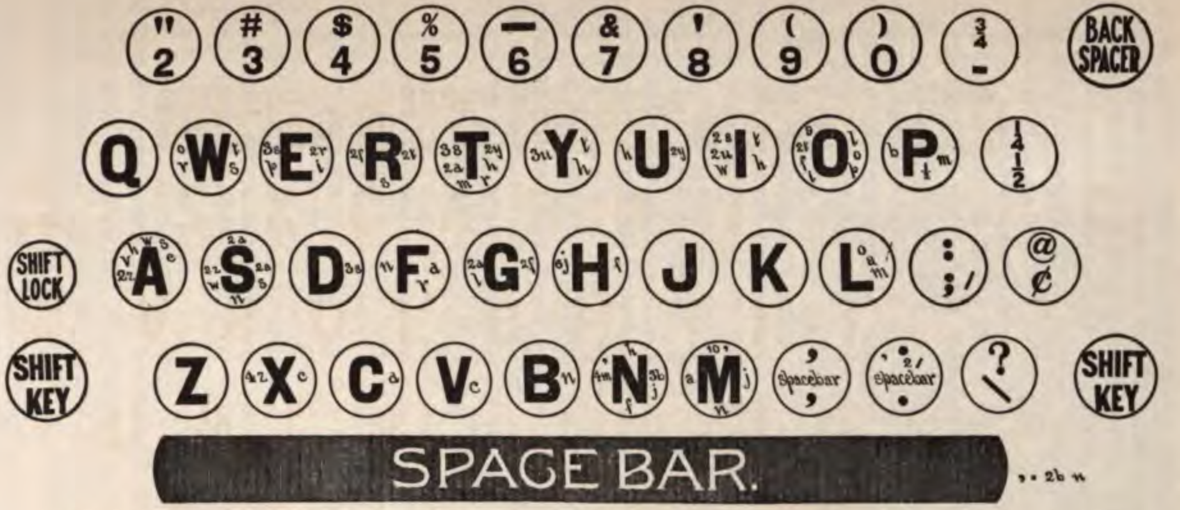
When neighboring letters are struck one is unable to know what is psychologically a very important thing, namely if the finger simply "dropped" off the key or if a wrong finger was used. It is certain that errors of both kinds occur.



Omissions by the two operators. A figure on the left of the letter indicates the number of times that letter was omitted by operator T, on the right, by operator J. Thus *e* was omitted twice by operator T, three times by operator J.



Substitutions by operator J. The large letters indicate the key which should have been struck; the small letters indicate the key which was erroneously struck. Thus *g* was twice written *k*, once *t*, and once *b*.



Substitutions by operator T. The large letters indicate the key which should have been struck; the small letters indicate the key which was erroneously struck. Thus *e* was three times written *s*, twice *r*, once *p*, and once *i*.

False strokes may result from the finger striking on a wrong bank of keys or being out of lateral adjustment, or the stroke may be in the corresponding position of the other hand, as the interchange of *e* and *i;* this is rare in the present material though a recognized type of error non-experimentally. The false stroke may also occur through a different finger with a different hand, as *n* for *r*, *w* for *u*, *e* for *m*.

There are doubtless errors which are determined essentially by the hands momentarily getting out of position through fatigue or some distraction. This however could produce only false strokes on neighboring keys and the fact that the false strokes are not in general " inaccurate " in a motor sense speaks further against this determinant as being of general importance. When a distant key is struck, as above, and with another hand, there must be a positive determinant for this particular key and it seems that the same factors at work here operate also, and more strongly, in the case of neighboring keys. It seems therefore that we should not speak of two types of errors, i. e., on neighboring and distant keys, but rather of two factors in the production of errors, one of which effects a mere dropping of the hands out of alignment and the other directs them positively towards a definite false stroke. These are not mutually exclusive but reinforce each other. Thus, a frequent type of error is the anticipation of a stroke really occurring later in the word and this seems to occur more easily if the anticipated stroke is in proximity to the stroke it replaces. Two typical cases are,

Error	Correct Form
mu$\overset{1.0\ s}{\ldots\ldots}$mor	humor
t r u$\overset{.9\ s}{\ldots\ldots}$u m phs [2]	triumphs

The mental process of anticipation is then more likely to express itself in a false stroke—that is, the factors of proximity and " distraction " reinforce each other in determining the false stroke.

In connection with these anticipations, or as they are called by students of linguistics, " regressive assimilations," and the transpositions or " metatheses," as they are technically known, a quite interesting finding appears. To quote from some analogous studies of the errors of speech and writing movements:

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[2] In this error a motor habit " tru " may also be effective. " *Überdeterminierung* " looms large in the mechanism of these errors.

"It will[3] be noted that very generally in the regressive lapses of Meringer and Mayer, and almost invariably in both the phonetic and graphic material of Bawden, the error is discovered, or, at least, so indicated, before the proper place of the erroneously made movement is reached. This is also universally the case with the writer's graphic material; the error is discovered before the proper place of the assimilated letter is reached by the pen. This fact opens to serious question the nature of the regressive assimilation. . . . Suppose the subject is to write *Engadin*. He writes *End;* now if he were to go on normally and write *Endadin,* there might exist a true regressive assimilation. But of this there are almost no instances. In practically all cases in which the error is not discovered before the proper place of the assimilated movement is reached the word appears as *Endagin,* a metathesis. It is probable therefore that most if not all regressive assimilations are really abortive metatheses, in which the error was discovered, or the primary memory of it lost, before the arrival of the second member. Something of the sort is indicated in those lapses quoted by Meringer and Mayer in which the speaker's introspection gives evidence that the apparent regressive assimilation would have been a metathesis if the error had not entered consciousness. Thus, '"*Ich werde auf das Ei . . . auf das Kreuz vereidigt*" (p. 35). Unkorrigiert hätte der Fehler so ausfallen können; "*Ich werde auf das Eiz verkreudigt.*"' And again, '"*Griebes . . . Liebesgram*" (p. 37); Ich glaube, ich wollte sagen *Griebeslam.*' This is one of the very rare instances in which the affected portions are not of the same extensity.

"The second member should therefore be considered as having, as such, no part or lot in the lapse. It will be noted that in most instances of the graphic metathesis, the affected elements are closer together than in the regressive assimilations, the relative separation of the movements in the latter being probably related to the greater time given for discovery of the error and change in the focus of consciousness. It will be noted that this latter condition is all that is needed to abort the metathesis, and that this may take place without consciousness of the error. The corollary of the non-existence of the regressive assimilation, save superficially, would seem to be obvious. If a certain movement is erroneously anticipated, that movement is not made again so long as the primary memory of it persists; a fresh start must be had, so to speak, the focus of consciousness must change, before the movement may be again attended to.

"There is, in fact, no case of motor metathesis which is not, in this sense, a progressive dissimilation, the second member being erroneously placed because of a consciousness of the individual movement-complexes, their number and identity, stronger than the consciousness of their order. When having made a linguistic movement prematurely we arrive at that space in consciousness which it should naturally fill, there is involuntarily substituted the movement that it displaced."

An examination of the present material confirms the above by affording an objective record of the temporal block between the two processes. If a stroke is written anticipatorily, that stroke is not made at the point from which it was anticipated without a definite block in the writing process somewhere preceding it. The cases which bear on this point are (in addition to *mumor* and *truumphs* above),

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[3] Wells, Linguistic Lapses, *Archives of Phil., Psych. and Sci. Methods,* No. 6, 1906, pp. 86, ff.

Error	Correct Form
.8 s 1.2 s (sp)..leas......s	leads
.3 .8 s i..t......f it	if it
.4 s 1.0 s m....icrosp...opic	microscopic
1.4 s gi......ittering	glittering
2.0 s ti e.....e	time
.6 s ecu......cation	education
1.2 s in t i.....itions	intuitions
1.4 s .4 s o......r.....om	from
.5 s co.....mestication	domestication
1.5 s ornat......ent	ornament
1.2 s .4 s idt.....enti....ty	identity
1 s w.....awakening	awakening
.4 s .6 s greath....(sp).....depth	great depth
.7 s indivud......ual	individual
1.0 s antu......quity	antiquity
.4 s ah....other	another
.8 s (sp)....gun......gus	fungus

Exceptional cases are:

Error	Correct Form
Jusus	Jesus
essentiam man	essential man
Imagintation	Imagination
they eyes	the eyes

If no block is shown on the smoked paper record then the regular result is a transposition. Accordingly most transpositions run off without blocks before or within the transposition, the cases on this point being as follows:

Error	Correct Form
exist,s	exists,
ture	true
parise	praise
happne	happen
raod	road

Thebias	Thebais
Brahim	Brahmin
(sp) try$\overset{2.0\text{ s}}{....}$(tyrranized, cf. below, p. 69	tyrannized
(sp) kindg$\overset{1.2\text{ s}}{.....}$om	kingdom
sia$\overset{.6\text{ s}}{.....}$d	said
infel$\overset{.3\text{ s}}{...}$x-	inflex- (ibility)
real$\overset{.8\text{ s}}{.....}$tion	relation

But a block may occur between the transposed letters, and yet a wrong letter be written in transposition as shown in the following:

Error	Correct Form
hod$\overset{1.0\text{ s}}{.....}$l	hold
alr$\overset{1.2\text{ s}}{.....}$am....(sp)	alarm
las$\overset{1.4\text{ s}}{.....}$w	laws
pr$\overset{1.6\text{ s}}{.....}$eform	perform
is$\overset{.8\text{ s}}{.....}$t	its

Transpositions occur between strokes of different fingers of different hands, of different fingers of the same hand and even of the same finger of the same hand. They are usually between adjacent strokes (*prosperiteis*), but not necessarily so, (*proterpy*). Nearly always they represent the interchange of two individual strokes. The writer has observed, not experimentally, but one instance (*unpredijuced*) where the patterns of two strokes are interchanged.

In speech and writing there are instances where a sound or letter is taken out of its correct place and put somewhere else, *amen* for *mean* and *sanct*$\overset{.6\text{ s}}{.......}$*urya*$\overset{1.2\text{ s}}{.......}$ (.) for *sanctuary*. The only other apparent instance of this in the present material occurs in the only word that itself shows a peculiar tendency to be written wrongly, *virtue*, the mistakes made in writing this being as follows:

Error	Correct Form
virtu$\overset{.6\text{ s}}{......}r\overset{.6\text{ s}}{......}$(sp)	virtue
viture	"
(sp) virtur	"
vitu$\overset{2.5\text{ s}}{......}$e	"
vitures	virtues

The essential thing about these errors is the tendency to write *ture* which, as a common suffix, is probably a motor habit phenomenon. But observe that the misplacing of the *r* tends to eliminate it where it rightly belongs.

The mechanism of this whole process would seem to be the motor phase of the "inhibition of similars" investigated and discussed principally by Ranschburg, on the sensory side. It is doubtful if it has produced actual "dissimilation" elsewhere in the present material, for the following instances, though showing it superficially, may be mere omissions with which the recurrence of the same stroke in the word had nothing to do.

Error	Correct Form	
remined	reminded	
no e	none	
creatues	creatures	
Jesur	Jesus	(substitutions)
no toc e	notice	

There is one case however that seems scarcely explicable on this ground,

Error	Correct Form
1.0 s .5 s 1.0 s consit.....tu....ional......ly	constitutionally

It is tantalizing to have to quote such a case without its introspective data, and the interpretation is ventured that the second *t* is put in because *t* was omitted where it should first have come and that the cause for the omission of the last *t* is intimately dependent on the superfluous one inserted two strokes before. The subject writing above expresses a sense of motor pattern for words in typewriting, thus, "I spell the word mentally, but the fingers travel faster than I can spell it. Sometimes I get through before the mind is through and come out one letter short." This gives a probable clue to the nature of some transpositions, the omitted letter most naturally stepping in to complete the number of strokes felt to be the proper one for the word. This seems most likely in such cases, as given above, where the last two letters are transposed.

Allusion has been made to cases where the error is not concerned with the substitution or transposition of the ideomotor processes for the strokes as such, but with some particular function of them, as where the doubling is transposed in *voicelles*. The present material shows cases of this in *tyrranized* and *morrally*, thus:

Error	Correct Form
(sp) orr$\overset{1.0\text{ s}}{.....}$ally	morally
(sp)$\overset{2.0\text{ s}}{......}ty\overset{.3\text{ s}}{.....}r\overset{.3\text{ s}}{...}$ranized	tyrannized
thses	these
(sp) Tu$\overset{.8\text{ s}}{....}$nker	Thinker

That the former at least is not an error of spelling is indicated by the circumstance that immediately before this error the word has been begun with the three strokes *try,* after which the operator went back and started the word over again. A more complex form of the same mechanism is seen in *thses* for *these.* This error involved no blocks, in fact it and the surrounding material were written at unusually high speed. Fusion appears involved in the case of *Tunker* for *Thinker,* the *u*-key occupying a position between *h* and *i* on the key board.

Mention has been made of errors apparently resulting from the motor habits of writing words more frequent to the operators. Instances of this are as follows:

Error	Correct Form
startin g	startling
the$\overset{2.0\text{ s}}{......}$t	that
spec$\overset{.4\text{ s}}{...}tab\overset{1.2\text{ s}}{.....}$le	spectacle
powe$\overset{.6\text{ s}}{...}t\overset{.3\text{ s}}{...}$'$\overset{.4\text{ s}}{...}$s	poet's
powet	poet

Insertions of superfluous strokes (additions), where they are not assimilations from the context appear to be generally produced in this way, e. g., *repulsiong, meantimes, wisdome, either* (for *ether*).

Three cases are worth quoting as illustrations of somewhat severer breakups in the adjustments. In writing the word *obedience* the operator first writes slowly *obec,* then back-spaces once and writes *dic,* then back-spaces again and writes *ecn.* Whether this last would have been corrected cannot be told as the writing period ended here and the operator was stopped. Characteristic is the irrepressible breaking through of the *c* before its time. The whole process goes on more slowly than the usual rate of writing.

For *the,* the strokes *fht* were written. They were not blocked, but somewhat slowed and the next word shows considerable delay between the strokes.

For *own* the strokes *ouw* were written as follows:

Error	Correct Form
ou$\overset{.5\text{ s}}{\ldots\ldots}w\overset{.4\text{ s}}{\ldots\ldots}(sp)\overset{2.0\text{ s}}{\ldots\ldots}$heaven	own heaven

Probably the *u* is a motor habit error (*our*) and the *w* is a belated correction, the *n* being kept out by the "pattern" sense of the number of strokes in the word. Immediately after *w* there was a block of .4 seconds before the space bar was struck and then a block of two seconds before the next word "*heaven*" was begun. There was no back-spacing although in this experiment the directions were not against it.

There was no noteworthy individual difference in the kind of errors made except that there are four times as many transpositions in operator J as in operator T. Only the study of further subjects could show if this fact was in some way inherent in different methods of writing used by each. With both operators substitutions are by far the most common, omissions next, additions next and transpositions rarest.

Practical conclusions may be stated as follows:

The experimental conditions caused in both operators a more rapid and less accurate writing than normal for them.

Under the non-experimental conditions both operators wrote at about 9/10 of the speed shown under experimental conditions; one operator made about half as many errors under non-experimental conditions; the other about 1/5 as many.

In the experiments both speed and accuracy average better at the noon period than at the morning one.

An unusual amount of work during the morning makes the noon efficiency lower in both speed and accuracy; it has a more deleterious effect on the accuracy than on the speed. The general gain in efficiency from morning to noon does not appear to be dependent on the doing of typewriting in the meantime.

It is reasonable to expect from these results that typewriting will be in all respects better done towards noon than at the beginning of the working day.

SOME DEVELOPMENTAL PSYCHOLOGY IN LOWER ANIMALS AND IN MAN AND ITS CONTRIBUTION TO CERTAIN THEORIES OF ADULT MENTAL TESTS

By Walter B. Swift, M. D., Boston

Before coming to the main body of this paper it may be well to review some unfamiliar facts of experimental psychology upon which I intend to base my theoretical superstructure. In this review I shall first present some data gathered from observation of dogs under training. Secondly, I shall describe some of the effects of vocal drill upon stuttering children. Next I shall record some of the steps in mental growth taken by a feeble-minded person who has long been under training for a defect of speech. Finally, I shall present some anatomical, neurological and neuropathological data collected in study of adult mental life, considered in a purely developmental aspect. Using these facts as a foundation, I intend to evolve from them several theories which they can be shown to warrant, suggest or imply.

1. *Center Development in Dogs.* Nine dogs were put under a uniform method of systematic training designed to teach them to differentiate between two tones. The process was as follows: The dogs were placed before a dish containing meat and then two notes were sounded—a low note and a high note. They were allowed to eat the meat at the sound of the low note but were punished for eating it when the high note was sounded. The training was continued until they developed the power to distinguish the two notes. They were trained three times a day for periods of about fifteen minutes and with the following results: At first they snapped up the meat without any regard to the two notes or to the punishment, but in a few days they began to show some hesitation after the sounding of the high note. This hesitation increased gradually until a fairly correct and constant reaction to the two tones had been developed. The time required for this development is important. On an average, it took fourteen days for the perfected reaction to occur with any uniformity.

We have here a condition reflex which, through severe experience or punishment and regular daily drill developed in two weeks into a firmly established reflex that differentiated tone.

Just what is this and how should it be interpreted? Some may consider it only a simple, low-type reflex similar to the knee jerk in human beings and other analogous reflexes well known to all. The characteristic of these reflexes is that they follow immediately after a sensory stimulation that is taken up on the afferent side of the nervous system, sent across the cord as rapidly as such impulses can traverse it and is expressed externally in a motion corresponding more or less closely to the level of the stimulus. It seems to me that these characteristics are quite different from those of our above-mentioned condition reflex. In the distinguishing between the two tones there was surely sensory intake, a crossing over of that impression to a motor area and an external expression in the attempt to eat. Thus we have stimulus, conduction and motor action, elements which are all found, to be sure, in the simple knee jerk, but we also have another element which is shown in a pause made by the dog long enough for him to distinguish between the two tones and to act according to the distinction made. Here, then, we have a holding up of the motor expression pending the result of a rather complicated mental differentiation, whereas, in the simpler reflex, we have merely an immediate response.

In this reaction, then, there is added a collaborative control. That is, there is an idea of differentiation that holds up the expression of the stimulus until the differentiation can be made.

This differentiation must be localized higher in the nervous system than mere sensory intake and output. For it is a developed function not exercised before; it can be retained in the memory for months after its attainment; its function is different from the functions of the lower centers. Choosing words general enough to cover the field of these data and their logical implications without inspiring criticism, I think I may claim that a new function or a new condition reflex has been set up, or I might say that a new form of activity has been developed which controls, through delay and direction, certain motor acts. In two weeks a functional center has been developed which controls, by interpretation of a sensory stimulus and by guiding a motor output, a certain developed reflex.

2. *Training the Stutterer.* We come next to a consideration of somewhat similar methods employed in treating a severe speech defect in man. The development in man is in many

ways like that which has been noted in dogs, but we shall have to note certain points of difference which will serve our present purpose better than data gathered from observation of dogs could do. In detailing the training of stutterers, I shall omit many points irrelevant to our investigation and shall do no more than outline the material, stressing those phases which should be borne most clearly in mind. First, I need to present some new psychological findings relative to the disease known as stuttering.

Some twenty or more stutterers at the Voice Clinic of the Psychopathic Hospital in Boston were subjected to a uniform set of visualization tests. Then there followed an introspection to ascertain whether there was any visual picture held in the mind previous to utterance, during utterance and after utterance. The relationship of the presence of stuttering to the presence or absence of these visualized pictures during utterance was carefully noted.

It was found that when stutterers stumble there is no visualization and that when stuttering is absent visualization is present. Upon the basis of this discovery a new method of treatment has been evolved which has been applied in the following manner:

Beginning with very simple methods of holding a visualized picture above utterance, we advance to more and more complicated processes of visualization coupled with certain exercises for concentration. I have noticed that in about two weeks these patients show what we might call a flowering out of mentality; that is, they acquire an ease of visualization which approaches almost to spontaneity, during speech. The situation is like the one we had before. A new function, or at least a more highly developed function, has been acquired which is amplified and extended in various ways so as to act as a new center of activity during the process of speech.

The result of this development of a new function is manifest. There occurs a delay in utterance and a waiting for the control of the higher center. A visual image is being built up and held during the process of utterance with the result that the image guides and controls the expression in speech. I have noticed that after the complete development of the new function there is never an absolute relapse to former conditions of spastic speech in cases which have been properly trained. We are justified, therefore, in the view that a new and permanently founded center of control has been developed. It is evidenced in new delays, new hesitations, new picture formations, new control of vocal expression and by an absence of the sudden, low-reflex utterance that occurred before the training. I feel justified in the claim that something which we might call a new brain area has been developed, a new functional center of speech control.

Comparing this situation with that observed in the dogs subjected to training, we find the following similarities: A previously uncontrolled reflex, a previously sudden action devoid of inhibition in any form is in both cases changed by about two weeks of drill of the collaborative centers into a greatly modified reflex which really deserves to be dignified by the name "condition reflex." This change is so marked that there is pause, consideration and interpretation of the stimulus and the action which follows is of such nature as to show conclusively that it is controlled and directed by higher developed centers.

3. *Mental Growth in a Mental Defective.* I wish now to describe the case of a defective boy who has been for two years under a very intensive system of vocal drill. Omitting numerous unnecessary and irrelevant details, it will suffice to say that the boy was eleven years old, that he belonged to a special class in school and that one chief symptom of his deficiency was indistinct speech. Before the treatment of the case began the boy spoke so indistinctly that only his own family could understand him and it was frequently necessary to repeat what was said to him. He measured 4 4/5 years by the Binet scale. He spent most of his time sitting about the house and was interested in little.

A very intensive vocal drill extending over two years resulted in a marked flowering out of the boy's mentality which is indicated in the following particulars which were noted in the order in which they are stated:

Slight improvement in attention and in speed of utterance. This increased speed is not shown, however, in the framing of fresh sentences.

His teacher remarks upon his improvement in reading.

His mother notices stronger and more protracted effort in all his work.

He becomes interested in the meanings of words and wants to have pictures explained to him.

He learns a second poem in two days as well as he did his first one in three weeks.

He shows more interest in general reading.

His mother reports that he is growing more observing, takes more interest in people, asks who they are and what they are doing.

He shows an increased interest in writing and submits a poem which he has written at his sister's dictation.

He uses the octave twist spontaneously in talking.

He improves at school and in conversation with the neighbors.

Memorizing the *Psalm of Life* proves too big a task for him.

He improves in speed in recitation.

A change is seen in his facial expression. It becomes brighter, more expressive, more responsive.

He reads long words in the newspapers, draws some pictures, writes more and develops several minor activities which he had never had before coming to the clinic.

He shows marked improvement in telling stories at the table, putting them into short sentences. This had never been done before.

He makes remarks about the dress of persons he meets.

He learns the names of some of the months and wants to know more.

He plays baseball.

His eyes grow brighter and his mouth hints increased strength of character.

He has more control over the movements of his body.

Increased powers of concentration enable him to learn more rapidly.

A summary and application of these steps will follow.

4. *Data Derived from the Location of Brain Centers.* Just anterior to the Fissure of Rolando in the brain is located what is known as the great motor area. This motor area controls the motions of the leg, arm and face. The area for the leg is at the top, that for the arm is in the middle and that for the face below. Opposite the arm area and a little way out into the frontal convolution is built up the area of writing. It is the function of the area of writing to control finer motions of the hand than can be managed by the motor area below it. You recall, also, that it requires a long time to build up this writing area. It is never inherited nor is it quickly acquired. There are, then, low motor areas governing the more generalized actions and high, built-up or painfully acquired motor areas which govern the more delicate and complicated actions. This distinction holds not only for the motor side of the brain's construction but for the sensory side as well. Just behind this same Fissure of Rolando is what is known as the great sensory area, divided in a way similar to that of the motor area. Above the arm region of this area, where sensations from the arm are recorded, is built up a higher area whose function it is to interpret these sensations. This function of interpretation is known in neurology as stereognosis, and when there is an involvement in this area a pathological condition results in which the patient is unable to interpet sensations. This condition is known as astereognosis.

Let us turn now to the cortical areas related to some sense other than that of feeling. Take, for example, the sense of sight. The low sensory impressions received through the eye are reported in the area of the occipital lobe known as the cuneus. Those low eye sensations are then interpreted by an area of the brain which lies around a corner, so to speak, from the cuneus. This function of interpreting sight impressions is called "psychic seeing." For a very simple illustration of this, take the following: I pass through the street and impressions of dark moving objects are made upon my retina and thence upon my cuneus. But I do not stop there. I interpret these impressions and sensations. I find that the dark moving objects represent people and I distinguish, among these people, between strangers and friends. That is, in neurological terms, I carry the sensation round the corner, outside the cuneus and into the area of psychic seeing.

If I were to try to picture the architecture of the brain on both its motor and its sensory side in a word, I should say: there are low sensory areas for the deposit of sensation and there are high sensory and motor areas for interpretation and guidance. The architecture of the brain is an intricate complex of forcibly evolved low-sensory areas, low motor-areas together with higher interpretative sensory areas and higher guiding motor areas.

It would be well, perhaps, at this juncture, to still clarify the points already made by referring to the pathological phases of brain anatomy and physiology. The scheme of cerebral architecture just given is confirmed by pathology. It is well known that when injuries to certain brain localities occur the functions of those localities are lost and that if degeneration results the functions never return. Suppose that a man has a hemorrhage that results from pressure over the motor area of the right side and that this extends into the inferior frontal convolutions and overlaps anteriorly, covering the back part of the central frontal convolution. This results, as you know, in a hemiplegia or one-sided paralysis of the opposite side of the body, with loss of motor speech and of the power of writing. In the parts of the brain just mentioned the cortical areas of motor speech and of writing are involved. Now when such a lesion occurs and the hemorrhage extends no further, the patient can hear what is said to him, can contemplate and think about what he hears, but he can utter nothing. In other words, his sensory speech area, which is in the middle of the first temporal convolution, receives impressions. These impressions are interpreted by the nearby areas and the interpretation is modified by certain thought processes. But when the patient tries to express himself in speech or writing his efforts are blocked. He can gesticulate and express his wishes or conclusions in this way, but as far as motor expression in words is concerned he is dead.

Suppose we locate the hemorrhage in another region, such as that of the occipital lobe. Suppose that we have a hemorrhage just outside of the cuneus and over the corner of the occipital lobe, in the area in which, as we have noted, the function of psychic seeing is exercised. If this area of nerve cells is destroyed there results an inability to interpret eye sensations which is known as psychic blindness. The patient can see his friends but he cannot recognize them.

I have adduced data from several different fields of investigation not with the intention of communicating new or even unfamiliar facts alone but with the design of bringing together the fundamental notions which I shall have to deal with later. What conclusions are we justified in drawing from the data already considered? The conclusions most germane to our present purpose are as follows:

The experiment with the dogs shows that a superior controlling center can be developed by drill even in the lower animals to such an extent as to dominate and modify actions under certain conditions.

The effects of vocal drill upon the stutterer show that an abstract speech-controlling center may be opened up which, by long and persistent work along certain definite lines, may be largely developed. There is a curious uniformity between the cases of the dogs and the stutterer in the fact that the arrival of the two developed centers was noted in about two weeks after training began.

The growth of the mental defective through long years of drill shows that an extended series of cortical centers,—low sensory, interpretative sensory, collaborative and controlling motor—can be forced through a long series of developmental periods. Watching this case step by step, we can almost see the arrival of the different functions in succession. The picture, moreover, is interesting as a sort of epitome of normal development. In its early years the child spends most of its mental effort in sensory registration. Later there is a period of sensory interpretation. By and by collaborative processes take the lead and, with them, a marked control and guidance of expression. I do not mean to imply that there is no activity prior to the completion of this evolution, but at first there is surely some sensory registration minus its motor expression. Then the motor expression develops along with all these other periods: First, sensory registration, together with a sort of reflex motor expression that lacks interpretation and collaboration. Then follow the growth of interpretative and collaborative processes respectively until finally a complex and intricately inter-related mental mechanism is built up. All this is to be seen in epitome in the case of the mental defect. His training acts upon his sensorium until reflex action results. Interpretation is built up and collaboration is added to it and the mental expression is dominated in turn by all the different stages of growth.

Anatomically, pathologically and experimentally located brain areas afford evidence that the whole cerebral system consists of a variety of centers, sensory and motor, with higher centers above each to interpret and control. This scheme of architecture is seen throughout the anatomy of the brain. The anatomical method of investigation locates these areas in specific parts of the brain and shows how the centers are built up near each other as well as their relationship to other centers. For example, we do not find the function of stereognosis near the cuneus or the function of motor speech in the temporal lobe. Stereognosis is located just above the arm area, where it should be. Motor speech is located just above the face area, where it should be. There is therefore a certain logical system in the location of these numerous cortical areas. And anatomy also gives us ground for assuming an inter-relation between these areas. We find that there are nerve fibres running from one convolution to the next or running from one convolution to the next but one or extending to more distant points on the same side. There are cross fibres which connect one side with the other. One author has gone so far as to say that all centers are related to all others. As we have seen, cerebral pathology provides evidence that supports this localization of functional areas and we can readily see, by the aid of pathology, how and why other areas are left free to act when the function of one area has been destroyed. Pathology also shows that even a few centers may continue to act after many are gone. The experimental method, used only upon the lower animals, offers further confirmation.

The intricacy, system and method of the brain and its functions provide one of the most beautiful examples of orderly construction to be found in all nature. Study of the brain is usually considered difficult or even uninteresting, but it ceases to be either of these when one brings to bear upon it an ability to reconstruct in the imagination its entire beautiful structure and its complicated inter-relationships. But, not allowing ourselves to be led away by the wonder of this picture, we should return to a consideration of the theoretical applications and practical results of the facts we have been considering.

1. *Theoretical Education.* These facts deserve application to several spheres of knowledge. I doubt whether educational

methods have in all cases been formulated and promulgated with a proper regard to what is known of the anatomy and developmental possibilities of the brain. Let me give one example. The Binet tests for intelligence are supposed to tell at what age a child should know certain things. In more scientific terms, Binet tests relate or compare the real age with the psychological development. These tests were based, originally, upon observations of French school children. When the system was transferred to America it was found that since American education follows somewhat different lines a modification of the French tests was necessary.

Now, if there is this difference between French and American children, there may also be a difference between every two children—a difference determined by the centers with superior interpretative and collaborative activities which have been especially or particularly developed. Therefore, I see no reason why another entirely new inter-relationship of developmental areas might not be instituted in infancy and worked up, with the result that the age type would be entirely changed. I see no reason why it should not be possible to conduct education solely along individual lines, leaving all others out. Such a process as this may account for prodigies of precocity, genius, and the like. Why, for example, can we not all have the exquisitely perfected development of the sense of hearing possessed by the blind child or the extraordinary activity of the cuneus of the deaf child? It is obvious that there are tremendous possibilities in this untrodden field and that their bearing upon education is of great importance.

2. *Type Study of Man.* Individual development along definite lines results finally in what we all agree in calling by the vague name "individuality." We hear, for example, of a "legal mind," and there might just as well be other terms such as "medical mind" or "psychological mind." There might even be a certain constant connotation behind the term "student mind." Suppose we analyze the meaning of that last term, "student mind." I choose it because it is less definite, less accepted, less formulated than the others. It may be well to narrow it a little and discuss the "Harvard type of student mind." The Harvard type of student is forced for four years along certain definite lines. They claim in Cambridge that they develop individuality. I may be allowed to criticize this view because I hold three Harvard degrees. What they actually do do is this: They force the student to listen, then they force him to write down what he hears, with little collaboration and with little effort to digest or think over

the material which passes through his ears and off the tip of his pen. At last the student is called up to another situation where the demand is made that he write the material all out again. Now, what sort of type is being developed here? A listening, slightly collaborating, writing individual. And this is the standard type of student put forth by Harvard. There are indeed many exceptions and a great deal depends upon the courses taken, but I think that in general my characterization of the typical Harvard student holds. He is a listening, slightly collaborating, writing individual.

I first became acquainted with this type of mentality while teaching neuropathology and ever since that time I have made a strenuous effort to modify the type into another and entirely different type. I thought of the matter in this way: These students take notes; they have little time to digest the subject matter; they simply write it down, and keep on writing it down for four years. Anatomically, this results in great activity of the hearing center in the temporal lobe, little collaboration in the higher centers, large development of the writing area—the listening, slightly collaborating, writing individual. It occurred to me to wonder what these students ought to be and I concluded that I should try to develop a type of student who saw, fully collaborated and talked; that is, a man who had developed his cuneus, his higher collaborative centers and his motor speech areas. The methods employed to this end need not detain us. I mention the matter only to show how the abundant data at our command may be used not only to describe or to treat the type of human beings now in the world but also to provide a basis for the evolution of better types in the future,—an educational application.

3. *Vocational Prophecies.* The data we have reviewed have an important bearing upon the choice of a vocation The different vocations demand different types of individuals and they find them only with difficulty. One cannot readily tell whether a given individual belongs to the required type or not. But if we could evolve some method of type registration which would describe the whole past mental constitution and all the inter-related activities of the present and all lines of possible future growth, we should have accomplished something of inestimable value both to the individual and to society at large. This would solve in a new way the pressing problem of the choice of vocation. But this is not the place in which to elaborate the matter.

4. *Ultimate Divisions of Consciousness.* The general prob-

lems of consciousness have, of course, been studied with great care, but certain important ones remain unsolved, unattempted. Southard has tentatively located consciousness in the post-pallium; that is, behind the Fissure of Rolando, but he makes no more minute localization of subdivisions. His statement is satisfactory as far as it goes, but on the basis of the data just reviewed we are justified in carrying the subdivision much further.

If our different sensations become conscious in different brain areas and these are widely separated and also inter-related and their inter-relations become conscious, then there is room for an elaborate division and for minute localizations of consciousness. Consciousness should be divided not only into low sensory areas, interpretative areas and collaborative areas, but into more minute subdivisions of each of these so that our ultimate divisions of consciousness would be those which correspond to all the different and separate brain localizations.

With all its ramifications and intricacies, this problem is extremely interesting and important, but we can do no more than mention it here as one deserving of more attention.

5. *Theories of Mental Measurements.* Of all the different fields into which it would be possible to carry the developmental data which have been reviewed, I have chosen only this final one for extended consideration. Even of this one I do not propose to make an exhaustive presentation but only to outline some of the theories and foundation principles that should dominate any complete discussion of the problem of adult mental measurements. I shall discuss the subject under the following three heads:

First, I shall construct a theory of cortical center relationships. Second, I shall suggest a theory of the inter-relationship of ideation components. Third, I shall present a theory of age relationships.

1. *A Theory of Center Relationships.* I have mentioned the dogs that showed development of one conditional reflex. I have mentioned the defective who built up in himself several related centers. I have mentioned other psychological developments which showed still more complex relationships between the centers developed. In all this there seems to be evidence that centers should be taken into consideration in mental measurements. Low sensory centers of perception should be considered, then the higher centers which interpret them should receive attention, then the collaborative centers

which work upon the interpreted sensations should be taken into the account. We have seen, moreover, that the motor expression of these processes is controlled by higher areas also, and these must not be omitted. Having, then, this serial view of cortical centers, we must necessarily consider all the functions of the individual mind—not only all its perceptions but all its interpretations, all its collaborative methods and all its inhibitions of output.

I do not wish to be hasty in discarding all of the many methods of mental testing now in vogue, but one can have little hesitation in saying, after careful examination of them, that they are limited, narrow and unsatisfactory. The Binet scheme, which is valuable in testing children up to the age of twelve or fifteen, is of doubtful value in the last two or three years. The Yerkes point scale which has improved the methods of testing children between the ages of twelve and fifteen is not applicable above the age of fifteen. Haines has recently presented a new system for use with the blind. Nute, of the Immigration Bureau, has another method which is adapted for use in the immigration service, and I have just heard that Haberman of New York has devised another scheme which differs from all the rest.

The problem has been attacked only recently and it is still too early to expect a perfect system. The mere fact that there are so many systems in use is a sufficient criticism upon them all. The rapidity with which new ones are devised shows that the old ones are unsatisfactory. The principal defect to be noticed in them all is that they have not taken into account all the possibilities of center growth. As systems, they have not been all-inclusive and exhaustive. Often they have fallen into the historical pitfall and have tabulated or systematized only the situation of the moment. By the historical pitfall I mean this: Hysteria was once diagnosed as a uterus running round the body; then came Babinski who said that hysteria is due to suggestion. Then came Janet, with an exclusively psychological eye, who held that hysteria is a limitation of consciousness. Now comes Freud, with a propensity for interpretation in terms of the subconscious, and he says that hysteria is an external manifestation of subconscious complexes. As another illustration, let us consider stuttering: This disease was once diagnosed as trouble with the tongue and the tongue was operated upon, a part of it being excised. Since that time the seat or location of the disease of stuttering has been pursued through both sides of the nervous system and into the brain and a new diagnosis and new form of treatment has been devised for every part. Last spring it was chased into the subconscious. This summer it has been driven into the thymus gland. This fall, as the result of a long psychological investigation, it has appeared for the first time in the conscious side of the mental makeup and in association with conscious collaborative processes. About the same thing may be said of psychiatry. First came the demonological theory and then the governmental conception. Next, there was a long period in which the clinical interpretation was dominant, but now the subconscious and psychological phases of these investigations occupy us almost exclusively.

I am not certain, but I suppose that the case is much the same with philosophy and that each philosopher's system is simply the psychologizing of his own individual type of mind. This is what I mean by the "historical pitfall." All along the ages students have been interpreting and explaining one man in terms of another. There have been so-called Platonists and Aristotelians for two thousand years. Thus all thinkers have been in the pit except those who have dared to climb over the edges and to see the broad horizon with their own eyes.

All this has its application to the mental test problem of today. It is very desirable that some one should outline and master the situation as a whole and, instead of tinkering at the systems already in use, devise one which shall include the good points in earlier systems, avoid the weaknesses and supplement the deficiencies.

At present we can do no more than outline the theory which should be followed in this matter, but the theory should be one that includes in its consideration all of the brain centers. I disagree with those who would begin their tests upon children at the age of two or three. There should be a complete tabulation of all that has been deposited in the sensorium up to that age. Has the child heard exquisite music for two years? Has he been subjected to an exquisite variety of smells so as to develop the corresponding sensorial area to a high degree? Have certain attitudes of individuals towards each other in the child's environment been so obvious, so repeated and so drummed in as to determine the child's later action when he comes to act for himself? Even these few suggestions will show the inadequacy that there is in beginning mental tests by tabulating the output at the age of two or three.

The theory of centers should really enable us to find the amount of sensorial intake, its length of time and its quality. This should be determined for all sensory areas. Then I should tabulate all the interpretative reactions upon these sensory intake areas, those that are spontaneous, those that are consciously taught and those that are forced upon the individual, whether accepted or objected to. After this, I should pass into the collaborative area and use a tabulating method to record all the processes and uses to which the intake material has been put. Finally, the motor control area should be studied in its methods of sifting, inhibiting and guiding expression.

We have been considering a theory of cortical centers and have spoken repeatedly of a relationship between cortical centers, but the exact nature of this relationship has not been discussed. It will be well to treat this matter more definitely.

2. *A Theory of Whole and Part.* I shall try to present an outline or method to be used in correlating exhaustive data tabulated from mental types. As an illustration, consider the painter who is in the act of composing a picture, in his mind. As a first step, he thinks the matter out as a whole. That is, he seeks a motive. This motive lies in a vague outline; it is the first conception, the first vague hint or intuition of the picture that is to be. His first step, then, is to select his *whole.* After this has been chosen and fixed, his mind turns naturally to the parts of his picture, mass, color, form, background, and in a more or less tentative way he selects certain details that may be included. In simple terms, this may be said to be his consideration of the *parts.* Next he thinks out the *relation of these parts to the whole.* He tries to make each of them serve and support the whole in some way. Finally he turns to what is perhaps the most difficult phase of his work. He begins to consider just how the parts *should be related one to another.* Thus we have a consideration of the whole, of the parts, of the relation of the parts to the whole and of the relation of the parts to one another as four pretty distinct processes which the mind of the artist must pass through, consciously or unconsciously, in order to complete his picture.

This may serve as a guide in our study of the theory of centers. We should begin by considering the individual or the type as a whole; then we should tabulate the parts of this type or individual as parts of the whole. Next we should tabulate the relationships of these parts to the whole and finally we should consider the relationships of the parts to one another. It is clear, at least, that a methodical procedure such as this must be far better than no method whatever.

3. *A Theory of Age Domination.* It is well known that mental measurements bear a definite relation to age periods. Viewing life as a whole, we are reminded of Shakespeare's seven divisions, but when we view it from the plane of the history of philosophy, the history of education, the history of practical business life, we are warranted in making certain deductions that do not harmonize with the division made in *As You Like It.*

The life of a normal human being who reaches old age is divided into four overlapping but distinct periods. Each of these periods is dominated by a different attitude towards the world and towards himself.

The first period lasts until puberty. It is dominated, as I see it, by sensorial registration. That is, registration of sensations upon the cortical areas is the dominant mental activity. Other mental activities are either undeveloped or are dominated and overshadowed by this one. There is little besides registration. This may be called, very vaguely, the "sense-period."

The second period begins at puberty and lasts for several years. It is marked by a profound mental disturbance as well as by many well-known physical changes. In this period a new center comes into activity which takes precedence over all others and guides and transforms the whole nature. This period may be described as affective, interpretative and it may be called the "period of affection."

In the third period another form of interpretation dominates. The affairs of life are taken up in relation to a developed personal self and they are decided upon, rejected or accepted in relation to that self's aims, hopes and ideals. In this period there is, of course, an element of each of the earlier stages, but these have lost their dominant and directing power. This may be called the "period of will."

The fourth period is that in which the collaborative processes of the individual finally become dominant. He is now, chiefly, a thinking being. All the other spheres or phases of his activity are taken up into this last one, but he regards them as mere avenues to this final goal or station of thought. He lives in those old shells of sensory registration, affection and will, to be sure, but he is no longer dominated by them. He is not led hither and thither by their overpowering influence. He is now led only by the workings of his developed collaborative processes. This period may be said to begin at the age of forty and to last perhaps to death. The period might be called collaborative, but we may dominate it more simply, the "period of intellect." Now let's put this all together.

I have tried to show that all these related theories serve in the formulation of exhaustive tabulations of mental tests which should cover the *entire life* with all of its variations. Mental tests should relate all sensorial content, all interpretative reaction, all forms of collaboration, all controlled methods of expressive output according to the correlations laid down in the theory of the whole and the parts. It should be kept in mind that these are dominated in the different life periods by sense, affection, will and intellect respectively, each of which is relegated in succession to its subordinate place. These should be related in such a way as to picture the past, present and future of all possible mental types in their perfected development and the percentage relationship should be shown between the present individual and his final, ultimate, developmental perfection. Thus I would summarize my paper.

So long as we confine our discussion to matters of theory we may picture to ourselves an ideal which is realizable, if at all, only in the dim future. This is what I have done. I grant that an exhaustive tabulation of really exhaustive mental tests is, according to our present knowledge at least, unthinkable. But theory works in the ideal. Practice, when we come to it, may lead into new scientific relationships.

FACTORS WHICH INFLUENCE THE AROUSAL OF THE PRIMARY VISUAL MEMORY IMAGE

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I. INTRODUCTION

The development of the problem of individual differences in imagery subsequent to the time of Galton has led away from the original simple solution to an increasing realization of the complexity of the whole matter. We note the failure of investigators to find simple types and the disputes as to the actual criteria of type. Fernald has recently stated that "individual differences in imagery are too complex to be stated in terms of differences in type unless this type is carefully explained in the individual case,"[1] and suggests that perhaps "individual differences may be more profitably stated without the incubus of types,"[2] while Lipmann[3] thinks that a further differentiation is necessary within the visual according as individuals best apprehend (auffassen) hue, saturation, brightness, size, or position.

In view of the present rather unsatisfactory status of the traditional problem of types, and of the nevertheless patent fact that individuals differ markedly in their mental imagery, the present investigation proposes to approach a limited aspect of the question within the field of visual imagery. The effect of certain fairly controllable factors connected with a visual stimulus upon the immediate arousal of visual imagery of that stimulus was studied. Complexity of contour, size, length of exposure of an object, interest aroused in an object, motor reinforcement by tracing, motor distraction and mental distraction during fixation were selected for investigation and their effect upon imagery of the object was determined. These are only a few of the many variables that might be made the subject of profitable study.

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[1] M. R. Fernald, The Diagnosis of Mental Imagery, *Psychological Monographs*, XIV., 1912 (No. 58), 130.

[2] *Ibid.*, 21. [3] O. Lipmann, Visuelle Auffassungstypen, *Bericht IV Kongres für experimentelle Psychologie*, 1911, 198.

The determination of imaginal types in the present case is wholly incidental. The interest of the experiment is four-fold:

1. How far do the above factors, operative during a period of visual stimulation, influence, in general, the arousal of imagery of the stimulus?

2. How far do individuals differ quantitatively in their susceptibility to these various factors?

3. How far do individuals differ qualitatively in the means by which these factors operate in influencing the arousal of visual imagery?

4. How far, if at all, are these factors interrelated?

Two previous experiments have dealt specifically with a few of the above factors. Meakin,[4] working with pairs of plane figures and noting the subsequent rivalry in imagery, finds that the larger or longer-exposed object or that with a notched contour persists in consciousness during a greater part of a given interval than does a simple object. Murray,[5] working with single exposures of simple figures, finds no correlation of duration or excellence of reproduction in imagery with complexity and size.

The other factors involved in the present investigation have been brought out only incidentally in previous experimental studies. Meakin[4] mentions interest as a cause for the more frequent occurrence of images of certain figures. Kuhlmann[6] using pictures of familiar objects as stimuli for imagery, concludes that the tendency for the image of a picture to become that of a real object is due to the fact that there is "more interest and emotional coloring to objects than to pictures." Martin[7] notes that, according to the introspection, the duration of imagery frequently depends on interest.

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[4] F. Meakin, Mutual Inhibition of Memory Images, *Harvard Psychological Studies*, I., 1903, 235-275.

[5] E. Murray, Peripheral and Central Factors in Memory Images of Visual Form and Color, *American Jour. of Psychol.*, XVII, 1906, 227.

[6] F. Kuhlmann, Memory Consciousness for Pictures of Familiar Objects, *American Journal of Psychology*, XVIII, 1907, 420.

[7] L. J. Martin, Die Projektionsmethode und die Lokalisation Visueller und anderer Vorstellungsbilder, *Zeits. für Psychologie*, LXI, 1912.

The motor element in visual imagery is repeatedly emphasized by Meakin and he concludes that the factors which he studied (such as size, complexity, broken lines, etc.) are the "conditions which determine the energy diversity, complexity and definiteness of the active process involved in the bestowal of attention upon its object, and . . . such active processes are as essential in ideation as in perception."[8] Kuhlmann, using meaningless forms which are memorized and recalled after an interval, finds motor tendencies in the imagery and these are in some cases "real aids to recall."[9]

Meakin mentions attention in the above quotation. Slaughter[10] notes that the distribution of attention in imagery and stimulus are similar. If a row of dots is followed across with the eyes, the dots can be brought out successively in imagery. Martin[7] states that, according to the introspection of the subjects, the duration of imagery often depends upon attention. Distraction has sometimes been employed in tests of imaginal type. The mode of distraction (visual, auditory, verbal) which produces the greatest effect upon the objective results of a learning "Aufgabe" is supposed to indicate that the subject belongs to the corresponding type. The method has been used with considerable success by some investigators, but it is sometimes found that the distraction has no effect upon the imagery.[11] In the present experiment, however, the method is somewhat different,—the distraction being employed during the stimulus period and the effect upon *subsequent* imagery noted.

II. APPARATUS AND METHOD

Each of the seven variables above mentioned was studied, in the present investigation, from three standpoints.

1. *Single* exposures were made of simple geometrical figures, some trials involving and others not involving the given variable. The time necessary for arousal and the time of involuntary holding of subsequent imagery under the former condition was compared with that under the latter.

2. *Simultaneous* exposures were given of two figures, one of which involved the variable under consideration. The rela-

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[8] F. Meakin, Mutual Inhibition of Memory Images, *Harvard Psychological Studies*, I, 275.

[9] F. Kuhlmann, Mental Imagery and Memory of Meaningless Forms, *Psychological Review*, XIII, 1906, 344.

[10] J. W. Slaughter, A Preliminary Study of the Behavior of Mental Images, *American Journal of Psychology*, XIII, 1902, 535.

[11] *Cf.* J. E. Downey, Central Processes in Modified Handwriting, *Psychological Monographs*, IX, 1908 (No. 37), 99.

tive predominance of the two in imagery was noted, and compared with the results of the check series in which neither figure involved the variable. The times of arousal and holding of the imagery under the two conditions were compared.

3. The *successive* exposure of two simple figures in check series usually yielded a marked predominance of the second in subsequent imagery. By introducing a hypothetically reinforcing variable on the first object or a distracting one on the second, the effect in reducing the predominance of the second could be noted. The times of arousal and holding of the image in the crucial and check series were compared.

The variables were controlled by the nature of the stimulus object or by the "Aufgabe" during the stimulus period. Figures with notched edges, with twice the usual area, with longer exposure, or figures representing in outline certain meaningful objects, were used to control complexity, size, exposure length and interest respectively. Motor reinforcement consisted in the subject's tracing a figure on the table while fixating it. Motor distraction was produced by writing extraneous words during fixation. Mental distraction consisted in performing mental addition while observing the figure.

An experiment of this sort necessitated an exposure apparatus that would provide a means of eliminating sensory after-images by a flash of light prior to the mid-period, and that would accurately control the time relations of the stimuli.

A few of the earlier series were performed with a relatively crude apparatus, Hering's *Nuancierungsapparat* adapted for the purpose. This consisted of a box 94 x 30 x 20 cm. with one of the broader sides open, and mounted vertically with the open side facing a north window. The subject sitting on a high stool with his forehead on a rest, could see, through a small aperture in the top of the box and through a diagonal piece of glass, the black rectangular field at the bottom. When the experimenter pulled a string, a black cover fell across this field and automatically opened a door on the side of the box which admitted light reflected from a piece of milk glass outside. This light was reflected upward from the diagonal piece of glass in the box to the eye. The stimulus object of black cardboard was placed on the field and at the signal "ready-open," the subject opened his eyes and fixated the object for 5 seconds until the experimenter, noting a stop-watch, pulled the string. As soon as the flash appeared the subject closed his eyes and observed the imagery that arose, signalling its appearance and disappearance by raising and lowering the finger. The experimenter took the time with a stop-watch.

The majority of the work, however, was performed with an enlarged and somewhat modified Dodge tachistoscope.[12] This consists in principle of a box with black interior with two exposure fields at opposite corners visible through an opening at the third corner,—one field, when illuminated, seen directly through a diagonal piece of smoked glass and the other, when illuminated, seen reflected from the glass which with dark background acts as a mirror. The present apparatus was 25 cm. deep with the 25 x 25 cm. fields 96 cm. from the rectangular opening hooded for the eyes of the subject. The fields were illuminated by light from two 25 watt tungsten lamps reflected by mirrors outside the range of vision. A third lamp was placed inside the apparatus just to the right of the smoked glass in such a position that its filaments were thrown out of focus on the eye by a 10 cm. condensing lens. A slit 70 x 8 mm. in a cardboard screen admitted a small portion of the light from the filaments. This was sufficiently strong to destroy the after-image, but not sufficiently so to cause discomfort. The lamps were on separate circuits and by proper control the stimulus objects appeared successively in the same apparent position followed by the flash of light. The apparatus rested on legs on a table with the hood on the level with the eyes of the subject sitting on an adjustable chair. The experimenter sat at one side of the apparatus at a table containing the switch-board, stop-watches and material for stimuli.

A slit was cut in the cover of the apparatus above each of the exposure fields to admit the insertion of pieces of 25 x 25 cm. cardboard with the stimulus objects pasted or drawn upon them. One field could be reached from the experimenter's seat, but the other could not without standing up and reaching across the apparatus. At this latter were installed four traveling frames of light brass suitable for holding stimulus cards. Their contiguous surfaces were perfectly smooth and they were operated from the experimenter's seat by strings and pulleys so arranged that any frame could be pulled into position and the others drawn to one side. The field was masked so that no brass was visible. The frames were loaded at the beginning of the hour's work and it was thus possible to avoid much confusion and save considerable time during the series.

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[12] *Cf.* R. Dodge, An Improved Exposure Apparatus, *Psychological Bulletin,* IV, 1907, 10-13.

The length of exposure of the stimuli was controlled by means of the circuits through the lamps. A wooden disc 22 cm. in diameter, mounted on an axis, was driven at the rate of 3 revolutions per minute by an alternating current motor reduced by belt gear. On this disc were mounted three brass contacts in the form of arcs of circles of different radii having the axle as the common center. The radius drawn to the end of one arc marked the beginning of the next, so that, as the disc revolved, they would in succession pass a given radius. Three contacts of spring brass, with their free ends in the same line were adjusted to press in succession upon the contacts on the disc. Thus during a part of the revolution the first spring contact was on the first contact on the disc; just as it reached the end of the arc and pressed against the wood, the second spring contact pressed on the second contact on the disc, etc. The contacts on the disc were connected with the axle and mounting and thus to one pole of the lamp circuit. The three spring contacts led through the three lamps respectively and joined at the other pole of the main line. Thus the three lamps were lighted in succession, one disappearing just as the next appeared. The first arc on the disc was stationary and of the proper length to give a 6 second exposure. The second was pivoted at the end nearest the first and held in position by friction in a narrow slit sawed in the wood along the arc. The length of the contact could be shortened by pushing the free end farther through the disc so that the spring that rested on it would touch merely the wood. The third arc was of the proper length to give a one-second exposure and was soldered to a strip of brass passing under a screw clamp at the axle so that it could be moved along in a shallow groove to a position immediately following the end of the second contact. Thus the two objects in the tachistoscope could be shown successively for various lengths of time followed by the flash of one second. When it was desired to use single exposures a switch in the circuit of the first lamp was opened. Other switches made it possible to reverse the order in which the first two lamps were lighted or to have one lamp lighted by both of the first two contacts. In part of the work, the motor and lamp-controlling mechanism set on a table with the tachistoscope, but later they were moved to an adjoining room and the wires led through the wall. The sound of the motor was never noted as a distraction by the subjects and furthermore it was a constant factor.

To record the time of arousal and holding of imagery two stop-watches were used. One was of the football type, started and stopped by plungers at the side. It was mounted with two short strips of brass, each pivoted at one end resting on the respective plungers. The free ends were connected by light steel wires through screw eyes to the armatures of two telegraph sounders. These latter was actuated by a make-and-break key operated by the subject. With the key in its normal position the sounder which pulled the lever against the "stop" plunger was in operation. When the subject pressed his key it broke that circuit and closed one through the other sounder which actuated the "start" plunger. When the key was released the original circuit was again closed. This watch was used to record the time of holding of the image. The other watch was of the ordinary stem starting type and was mounted horizontally with a strip of band iron pivoted a short distance to one side of the stem. Opposite the end of this was a horseshoe electromagnet which could be operated by two parallel circuits. The first of these was closed by a contact at the proper point on the rotating disc which controlled the exposures in the tachistoscope,—a piece of brass on the disc brushing across two poles 2 mm. apart. The second circuit was closed by a relay in connection with the sounder which started the first watch. Thus the watch started automatically just as the second object disappeared in the tachistoscope, and stopped when the subject signalled the presence of the image by pressing the key.

The materials for the experiment consisted, except in the series on interest, of simple geometrical or meaningless figures. None of the polygons had more than six sides and the curved figures were comparatively simple. The figures were uniformly of 36 sq. cm. area with the exception of some of twice that area used in the study of the effect of size. Those used in the "*Nuancierungsapparat*" were cut from black cardboard and were exposed upon a field of the same material. Those employed in the Dodge tachistoscope were of white paper pasted on black cardboard, or were drawn in outline wth ink on light grey cardboard. Those used in single and successive exposures were centered on the card. Those used in simultaneous exposures were side by side with a space of 2 cm. between their nearest points. In the latter case the space error was always obviated by presenting the two figures in both space orders in different trials. Similarly the time error on successive exposures was controlled,—the same figure occurring first in half of the trials in which it appeared, and occurring second in the other half.

The instructions given to the subjects in previous experiments involving a definite visual Aufgabe have varied considerably. Meakin[13] and Murray[14] instructed their subjects to await passively the entrance of the image into consciousness. Martin[15] and Ogden[16] told theirs to "get an image." Perky's instructions[17] were to be on the lookout for images. In the present experiment the subjects were given two hours of preliminary training in which they were instructed to "call up a visual image of the object just seen, observing it as passively as possible and signalling its presence by pressing the telegraph key and its disappearance by releasing the key." These instructions rapidly passed into a determining set and in all subsequent work, after being told to fixate the figures that appeared or to fixate between them (on simultaneous exposures) or to perform the required operation during fixation, the subject was merely instructed to "close the eyes after the flash and observe whatever imagery arises, pressing the key when the image appears and releasing it when the image disappears." These instructions were given at the outset of the hour's work and not repeated during the series. After each trial the subject described the image and the introspection was recorded verbatim.

Visual stimuli seemed more suitable for an experiment of this sort than word stimuli, for they afford a better objective control of the variables under investigation. Word stimuli would in the present case be more liable to introduce extraneous factors such as suggestion. Moreover the visual stimulus affords a better control of the subject's state during the foreperiod, for he is always observing a figure for 5 or 6 seconds prior to the mid-period in which the image appears.

That after-images did not play a part in the experiment seems evident from the intensity of the flash that followed the exposures. The subjects frequently noted a momentary after-image which was "wiped out" by the flash. Furthermore the qualitative aspect and the temporal course of the images for a given subject were fairly similar whether the stimulus was a white figure on a black field, a black cardboard figure on a black ground or a figure drawn in outline with ink on a grey card.

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[13] F. Meakin, Mutual Inhibition of Memory Images, *Harvard Psychological Studies*, I, 1903, 237. [14] E. Murray, Peripheral and Central Factors in Memory Images of Visual Form and Color, *American Journal of Psychology*, XVII, 1906, 229.

[15] L. J. Martin, Die Projektionsmethods und die Lokalisation Visueller und anderer Vorstellungsbilder, *Zeitschrift für Psychologie*, LXI, 1912, 329. [16] R. M. Ogden, Experimental Criteria for Differentiating Memory and Imagination in Projected Images, *Psychological Review*, XX, 1913, 379.

[17] C. W. Perky, Experimental Study of Imagination, *American Journal of Psychology*, XXI, 1910, 428.

In some of the previous work of other experimenters the imagery has been observed during a definite interval,—frequently a minute,—the successive appearances of the image being recorded. The present work dealt only with the primary visual memory image. Subsequent recurrences after the first disappearance of the image were not taken into consideration. In one series the times for the primary and for the recurring image (i. e. the total time the image was present during a minute) were recorded for four subjects,—about 60 trials each. The coefficients of correlation between the two times by the Pearson product-moments formula were:

.6056+.063
.4710+.089
.4298+.089
.4004+.075

If as satisfactory results can be obtained by using the primary image which lasts from 1 to 20 seconds as by using the recurring images for a minute, the great saving of time is evident.

The experiments were performed in the Harvard Psychological Laboratory during the academic years 1913-14 and 1914-15. Fourteen subjects participated at various times in the work. Of these one was an instructor in the department, nine were graduate students (two of them women) and three were undergraduates of considerable psychological experience. The entire experiment comprises reports on approximately 5500 images.

The writer expresses his sincere obligations to Professor Herbert S. Langfeld and Professor Hugo Münsterberg.

III. EXPERIMENTAL RESULTS

A. *Complexity of Contour*

The results of the experiments on complexity of contour are summarized in Table I. The first column gives the subject. The next two give the results for single exposures,—the % superiority[18] of the notched series to the simple, i. e., the % by which the average time of holding is greater, and the difference between the two averages divided by the probable error of difference. The next six give the results for simultaneous exposures. The introspective accounts fall readily into trials in which the notched or simple figure predominates in imagery (i. e., is clearer or more persistent) or in which the two figures are equal. The table gives the % of the trials in which the notched predominates and in which the simple does so. Then follow the % superiority of the notched series to the check series (with both figures simple) in times of arousal and holding, i. e., the % by which the average time of arousal is less and the average time of holding greater, with the respective differences divided by the probable error of difference. The remaining columns give the results for successive exposures. The % of trials in which, according to the introspection, the figure exposed second predominates or appears alone in imagery in the series with the notched object shown first, is divided by the corresponding % for the normal series with both objects simple. A smaller ratio in the table indicates a greater effect of the complex first figure in reducing the natural predominance of the second figure. Then follow the % superiority of the series with the first figure notched to that with both figures simple in times of arousal and holding of imagery, with the corresponding differences divided by the probable error of difference. The temporal aspect of the image in the simultaneous and successive methods is not as important as the qualitative. It merely indicates whether the images rise more quickly and hold longer when one of the objects shown involves the given variable than when both are simple,—a factor measured more directly in single exposures.

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[18] Percents reckoned in terms of the smaller figure throughout the work.

The table shows that with 6 of the 7 subjects who participated on single exposures, the notched image holds longer on the average, than the simple, with an average superiority, including the negative case, of 11%. On simultaneous exposures the notched figure predominates in imagery in 69% of the trials as compared with 9% for the simple figure, while in the check series (not given in the table) the majority of the trials yield images that are equal. The images rise more slowly and hold longer on the notched series than on the check series.[19] In successive exposures the second figure predomi-

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[19] It may be noted in passing that the whole series of experiments on imagery indicates that the time of holding is a much better criterion by which to judge the influence of various factors upon imagery, than is the time of arousal. The two times were correlated for 10 subjects on about 140 trials each by the Pearson produce-moments formula. There were three appreciable negative coefficients (.23, .30 and .34) and one positive (.29) while the others were small. In many cases the subjects get into a certain rhythm of arousal which is little influenced by the character of the stimuli. The time of holding correlates more often with the introspection.

TABLE I

COMPLEXITY OF CONTOUR

Subject	SINGLE EXPOSURE		SIMULTANEOUS EXPOSURE						SUCCESSIVE EXPOSURE				
	TIME		REPORT		TIME				REPORT	TIME			
	HOLDING				AROUSAL		HOLDING			AROUSAL		HOLDING	
	Percentage of superiority notched	Difference / P. E.	Notched predominate	Simple predominate	Percentage of superiority notched	Difference / P. E.	Percentage of superiority notched	Difference / P. E.	Notched / normal	Percentage of superiority notched	Difference / P. E.	Percentage of superiority notched	Difference / P. E.
Br	—3.0	1.6											
Bz			75%	17%	—27.2	3.8	18.9	4.6	.40	16.2	2.7	0	0
C	14.2	1.5											
D			96%	4%	17.4	1.6	64.4	7.9	.12	—8.1	.8	14.1	3.1
F	6.3	.9											
H	15.8	1.1											
L	28.8	1.5	25%	12%	—70.8	14.7	21.0	4.7	.43	—7.1	1.7	64.1	9.8
Ms	2.7	1.0											
Mt	10.0	.8											
R			80%	5%	-290.0	12.8	40.4	7.0	.43	22.7	1.7	4.5	.6
Average........	10.7	1.2	69%	9%	—92.6	8.2	36.2	6.1	.35	5.9	1.7	20.6	3.3

nates in imagery about .35 as frequently when the first figure is notched as when both are simple, and the images in the former case rise somewhat more quickly and hold considerably longer than in the latter.

As a further light on the single exposure method a series was performed in which the visual stimuli were not presented. The subject sat in the same position as in the previous series with closed eyes and after the signal " ready " the experimenter named the object,—" notched square," " simple square," etc. The subject was to call up a memory image of the object as he had seen it a week before and signal its presence in the usual manner. The results tend in the same direction as the above,—the complex images holding longer for 5 of the 7 subjects with an average superiority of 8.5%.

The qualitative aspect of the results in some cases throws light upon the quantitative. There are, of course, individual differences in the color, clearness, position, etc., of the image and in its mode of appearance and disappearance. Such facts, although of general interest, do not concern the present problem. Mention will be made of only those points of introspection which indicate the effect of the variable under investigation.

On single exposures with *Br* kinaesthetic and affective factors seem prominent. The notched figures are frequently reported as " clear and pleasant." The notched circle, however, is inferior in time of holding to the simple and is described as " vague kinaesthesis going around; not as pleasant as the plain circle; little jar as if rolling along and the rolling jerky; sort of kinaesthetic jar." There is also a " tendency to look around it; less of this in the image." *F* occasionally reports associations in connection with a figure that holds rather long in imagery. *H* notes that it is " easier to attend to the notched figure, for there is more complexity." *L* is very kinaesthetic and often thinks of going around the figure or notes a " kinaesthetic image on a visual background " or a figure " filled in with kinaesthesis," or " intimation of teeth on top and kinaesthesis below." This kinaesthetic aspect seems to correlate with the longer holding of the notched image. However in the check series with word stimuli, he experiences great difficulty in getting an image at all of the notched figures. *Ms* notes on the notched circle, " Revolved; associated with pinwheel " or " Associated with circular saw; teeth slanted." This associative factor tends to lengthen the time of the image. In the check series with word stimuli the reports are somewhat similar to the preceding. *C* and *F* note movement of the eyes about the image of the notched figure. *L*, as just mentioned, has difficulty in calling up images of the notched figures and such images are less clear, slower of arousal and less persistent.

In simultaneous exposures *Bz* sometimes reports only the middle part of the two objects. This would seem to follow with the fact that the subject fixates between the two figures and the inner parts are consequently more in the focus of attention. *D* notes, " I try to divide the attention but I think the ragged one attracts me and the other fades first." *L* also shows a tendency for the portion nearest the point of fixation to be most marked in imagery. *R* states that the " notched ones were interesting."

On successive exposures *Bz* states that the notched ones are more interesting. With *L*, when the first object appears in imagery (as it does only in the notched series), it is frequently in kinaesthetic terms. In a number of trials the second takes on the notched attribute of the first. For example, " second with wavy lines ; " " First, then second inside it crumply."

B. *Size*

The results of the experiments upon the influence of size are summarized in Table II which is identical in form with Table I except for the substitution of " large " for " notched " and " small " for " simple." The table shows that in single exposures the images of the larger figures hold 11% longer on the average than those of the small. On simultaneous exposures the large figure predominates in imagery in 32% of the trials as against 22% for the small figure, while the check series (not given) usually yields both images equal, and the images rise more slowly and hold longer on the series involving large objects than on the check series. On successive exposures the second figure predominates in imagery .76 as frequently when the first figure is larger as when both are of equal size and the images in the former case rise, in general, more quickly and hold longer.

As a further check on the single exposure method a series was performed in which the visual stimuli were not presented. The subject sat in the same position as in the previous series with closed eyes, and after the signal " ready " the experimenter named the object,—" large square," " small square," etc. The subject was to call up a memory image of the object as he had seen it a week before and signal its presence in the usual manner. The results tend to substantiate the above,

TABLE II
Size

Subject	SINGLE EXPOSURE		SIMULTANEOUS EXPOSURE						SUCCESSIVE EXPOSURE				
	TIME		REPORT		TIME				REPORT	TIME			
	HOLDING				AROUSAL		HOLDING			AROUSAL		HOLDING	
	Percentage of superiority large	Difference / P. E.	Large predominate	Small predominate	Percentage of superiority large	Difference / P. E.	Percentage of superiority large	Difference / P. E.	Large / normal	Percent of age superiority large	Difference / P. E.	Percentage of superiority large	Difference / P. E.
Br	8.0	1.3											
Bz			42%	42%	—15.3	3.3	24.4	12.2	.90	29.8	5.7	24.6	6.9
C	10.4	1.2											
D			4%	4%	1.5	.2	52.0	8.0	.91	19.4	2.2	—20.0	3.4
F	1.3	1.6											
H	44.3	1.0											
L	9.2	2.4	31%	19%	—84.0	16.4	15.7	4.4	.58	—14.0	2.7	57.2	7.1
Ms	10.4	.9											
Mt	—6.5	.6											
R			50%	25%	-178.0	11.5	25.0	5.0	.63	72.2	3.9	—5.3	.8
Average........	11.0	1.3	32%	22%	—68.9	7.8	29.2	7.4	.76	26.8	3.6	14.1	4.5

the image of the larger object holding 10% longer on the average.

A few points from the introspection may be noted. On single exposures with *Br* there is a possible correlation of time of holding with affective tone, the large figures being frequently reported as more pleasant. *C* notes that the large image sometimes " swells up " or " spreads out " at the end. *H* is " conscious of the effort to see the whole image; the eyes seem focussing one part and then another." *L* notes occasional kinaesthesis with the large figures. *Mt* states, " In looking at the object I tend to draw the part that interests me and the image has the heavy black line I have drawn." In the series with word stimuli *C* reports the large images much larger than the original. *F* notes the images as tending to build up. *L* finds the large images more difficult of arousal, e. g., " Mostly kinaesthetic; felt self pushing it out; tendency to make it small first; the pushing seems to be in the eye muscles; larger it is the longer it takes to get it." For *Ms* the large images are much larger than the original, often starting large and growing smaller.[20]

In the simultaneous series *Bz* gets " the smaller figure as a whole, while with the larger it is a process of building up."

On successive exposures *L* notes occasionally the image of the second figure larger than the original. This is doubtless due to the influence of the first figure. He speaks of carrying in mind the difference in size.

C. *Length of Exposure*

The results of the experiments on length of exposure are summarized in Table III. Its form is like that of the preceding tables. The exposures were 5 and 10 seconds for the single exposure method. For the simultaneous, the apparatus was arranged so that one figure appeared alone and then the other beside it. A piece of 6 mm. board 25 x 10 cm. and painted black, was hinged to the edge of the tachistoscope with spring hinges in such a way that when a black thread was held taut one stimulus figure was obscured and when the thread was released the shutter flew back against the wall. The electrical connections were changed so that both contacts on the disc governing the exposures lighted the same lamp of the tachistoscope. The experimenter held the thread taut during the 6 seconds on the first contact and released it, at the slight flicker when the contacts changed. Thus the left stimulus appeared for 6 seconds and then both for 6 seconds. The subject fixated the first object and when both appeared fixated between them.[21] In the table are given the per cent of trials in which the longer exposed of the two predominates and in which the shorter does so, with the per cent superiority in times of arousal and holding of the present series to the check series with the objects shown together an equal length of time. On successive exposures the variable was introduced by showing the second figure for one second and comparing such trials with the normal successive exposures of six seconds each.

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[20] It may be noted, as of methodological interest, that word stimuli have a greater effect in emphasizing an attribute of a figure than do the visual stimuli.

The table shows that for single exposures the longer exposed object holds some 10% longer in imagery on the average, although 3 of the 7 subjects show slight negative tendencies. The average results on the simultaneous method are identical for the long and short series. This is due to two subjects, *D* and *L*, whose introspection (infra) indicates the entrance of another factor. The average difference in the temporal aspect of the image under these conditions is slight. On successive exposures the second figure predominates about .81 as frequently when its exposure is shortened to one second as when both are shown for 6 seconds. The times are not given in the table as they show only slight differences in both directions. If, however, the holding time on the 6 and 1 second series is evaluated for the trials in which the first object predominates compared with those in which the second does so, the average of the former is 18% superior to the latter with the differences 2.4 the probable error on the average. In other words the imagery does not hold as long when the object exposed for 1 second predominates as when that exposed for 6 seconds predominates,—a result similar to that found with single exposures.

A few points from the introspection may be noted. On simultaneous exposures *A* frequently gets an "exact reproduction of the original; first one and then both." Evidently the distribution of attention in the image follows that in the stimulus. *Bz* says, "I take in the first one more quickly than the one beside something already in attention; feels as if the

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[21] No other method of studying this factor from the simultaneous standpoint seems available. Showing one object alone in the center of the field followed by the same object with another beside it has merely the effect of successive exposures, and the first, when it does appear in imagery, is in the center like the original stimulus. Showing both objects and then cutting out one would leave the effect of recency which is pronounced on all the work on successive exposures.

TABLE III
LENGTH OF EXPOSURE

Subject	SINGLE EXPOSURE		SIMULTANEOUS EXPOSURE						SUCCESSIVE EXPOSURE
	TIME		REPORT		TIME				REPORT
	HOLDING				AROUSAL		HOLDING		
	Percentage of superiority long exp.	Difference / P. E.	Long exp. predominate	Short exp. predominate	Percentage of superiority long exp.	Difference / P. E.	Percentage of superiority long exp.	Difference / P. E.	Short / normal
A			45%	10%	—29.0	.9	—15.7	1.4	
Br	11.2	2.2							
Bz			100%	0%	.4	.1	—22.5	7.1	.86
C	17.0	1.6							
D			0%	100%	7.8	1.3	21.1	3.7	1.00
F	—5.5	.5							
G									.26
H	36.5	1.1							1.00
K									.46
L	—2.8	.2	15%	77%	—2.6	.5	21.0	2.2	1.03
Ms	17.2	1.6							
Mt	—4.4	.7							
R			49%	22%	6.1	.7	—2.1	.4	1.12
T									.69
Average....	9.9	1.1	42%	42%	—3.5	.7	—.4	3.0	.81

attention swung back to it after closing the eyes; fact it has been there longer turns the attention to it." *D* always gets the shorter exposed figure clearer in imagery. In the early trials it is covered with a "veil," but the image of that one is nevertheless clearer. Toward the end the veil is "not present so much as its effect in making the image blacker and attracting attention to it." Probably the stimulus to attention of the screened objects accounts for the results. "Knowing it is screened arouses curiosity and interest in it when it comes; there is a tendency to go to the veiled object when it appears, although I try to fixate between them." *L* reports a "decided feeling of something coming in; I fixate the first and when I move to the middle it throws the attention to the other side; I feel the attention go." The results of *D* and *L* thus cannot be considered as evidence against the positive effect of exposure length, for owing to the technique of the experiment other variables entered with them,—in the case of *D* the factor of interest in the concealed object which is to appear, and with *L* the overshooting of the motor impulse or attention in turning from one figure to the mid-point between the two.

On successive exposures with *Bz* the effect of shortened exposure is more marked than the table indicates. In the trials where the second figure predominates he states that "it is not as dominant on the shortened exposure; in the normal, the first object appears in imagery only at the end, whereas with the shortened exposure it appears relatively earlier." With *D* the second object is relatively clearer in imagery in the series with shortened exposure,—a result the opposite of the general tendency. The following introspection explains the fact: "During the second exposure I recall the first by kinaesthesis; on the short exposure the first does not have time to come into relation with the second; the kinaesthesis of the first cannot arise during the second as it usually does." *L* reports that with the short exposure the images "come immediately and are more involuntary than with the longer exposure." *K* shows a similar tendency. With *Ms* and *T* one figure carries associations and tends to predominate whether shown first or second.

D. *Interest*

The results of the experiments upon the rôle of interest are summarized in Table IV. Its form is identical with that of Table I with the interesting figure substituted for the complex. The table shows that on single exposures the images

TABLE IV
INTEREST

Subject	SINGLE EXPOSURE		SIMULTANEOUS EXPOSURE				SUCCESSIVE EXPOSURE				
	TIME		REPORT		TIME		REPORT	TIME			
	HOLDING				HOLDING			AROUSAL		HOLDING	
	Percentage of superiority interesting	Difference / P. E.	Interesting predominate	Other predominate	Percentage of superiority interesting	Difference / P. E.	Interesting / normal	Percentage of superiority interesting	Difference / P. E.	Percentage of superiority interesting	Difference / P. E.
Br	—15.0	3.4									
Bz			75%	5%	12.2	4.7	.25	—7.5	1.3	3.4	.8
C	.5	.1	45%	28%	—14.8	4.4					
D							.58	—.5	.7	14.6	2.9
F	13.0	1.9	55%	7%	—46.3	11.4					
H	—3.9	.4	10%	2%	20.3	.5					
L	—1.0	.2	68%	16%	9.7	2.6	.21	—6.7	.4	8.1	.4
Ms	40.0	7.9	33%	13%	88.2	13.1					
Mt	—11.8	1.5	42%	12%	—41.0	10.6					
R							.30	56.0	4.8	15.5	4.6
Average..	3.1	2.2	47%	10%	4.2	6.6	.33	10.3	1.8	10.4	2.3

hold somewhat longer for the meaningful figures. Some of these figures, however, were much more interesting than the others. If the results for the three most interesting stimuli are averaged together, they hold 18% longer than the average of the normal series. In simultaneous exposures the interesting figure predominates in imagery in 47% of the trials as against 10% for the meaningless figure, whereas in check series with both figures simple, the majority of the trials yield equal images, and the images hold 4% longer in the former case. On successive exposures with the first object interesting the second predominates in imagery only .33 as frequently as when both are meaningless, while the images rise more quickly and hold longer under the former condition.

The following points in the introspection may be noted. In single exposures associations are frequent in connection with the imagery of all but the two most kinaesthetic subjects *L* and *Mt*. The figures that yield for a given subject the most associations are usually held by him longer in imagery than the other figures. With *F* there is a possible correlation between the time the image holds and the pleasantness of the associations. With *Br, C* and *Ms* the images tend to become the real objects represented instead of the plane figures.

In simultaneous exposures the factor of interest is mentioned by all the subjects. With three, *Br, Ms* and *Mt,* there is a tendency for the images to become real objects. There is frequently a change in predominance of an interesting object as the series progresses and the same object occurs several times. Toward the end of the series the images tend to be more nearly equal. This occurs with five subjects. As *Br* says, " The reason they even up in clearness is because at the beginning there is more difference in meaning. As I go along with it, the one tends to lose some of its associative value through repetition and the other tends to get a meaning. You exhaust the interest of one object and try to get interest for the other." Or as *H* states, " One object attracts the attention more at first, but after becoming familiar with it, it is easier to distribute the attention."

On successive exposures *Bz* states that " the more interesting it is the more it gets into consciousness." *Bz* and *R* have a tendency for the outline figures to become real objects in imagery. There is further a tendency with three subjects for the interest to wear off with repetition and affect the imagery. Considering the successive trials the second figure predominates more frequently toward the end of the series.

TABLE V
MOTOR REINFORCEMENT

Subject	SINGLE EXPOSURE				SIMULTANEOUS EXPOSURE						SUCCESSIVE EXPOSURE		
	TIME				REPORT		TIME				REPORT	TIME	
	AROUSAL		HOLDING				AROUSAL		HOLDING			HOLDING	
	Percentage of superiority traced	Difference / P. E.	Percentage of superiority traced	Difference / P. E.	Traced predominate	Other predominate	Percentage of superiority traced	Difference / P. E.	Percentage of superiority traced	Difference / P. E.	Traced / normal	Percentage of superiority traced	Difference / P. E.
Bs	—8.6	2.2	1.3	.2	95%	0%	10.7	2.2	24.4	4.9	.14	97.0	26.0
Bs											.09*	33.3	4.7
D	9.0	1.5	31.0	7.2	100%	0%	—36.2	3.4	10.8	2.0	.04*	2.6	.4
F											1.01	—62.8	12.7
H											1.00	—72.4	16.5
K											.69	—51.3	5.1
L	16.0	3.8	1.2	.3	40%	45%	—5.9	2.2	9.6	1.9	.91	—40.0	6.1
Ms											.08	14.2	4.2
R	—23.0	1.9	31.0	3.2	80%	10%	—260.0	11.1	6.5	.9	.74	—8.5	1.3
R											.42*	13.0	2.1
T											.67	300.0	23.0
Average.........	—1.4	2.3	16.1	2.7	79%	14%	—72.8	4.7	12.8	2.4	.53	20.2	9.3

* Check series; performed later.

E. *Motor Reinforcement*

The results of the experiments on motor reinforcement produced by tracing with a pencil on the table the outline of the figure fixated, are summarized in Table V, which is similar in form to the previous tables. On simultaneous exposures the subject fixated between the figures while tracing one of them. The table shows a slight average difference on single exposure between the tracing and the normal series in times of arousal, but a 16% superiority of the latter in time of holding. On simultaneous exposures the traced object predominates in 79% of the trials as compared with 14% for the other object. The images in the tracing series rise much more slowly and hold considerably longer than in the check series with no tracing. The delay of the images on the tracing series is probably due to the confusion of having in the hand the pencil with which the tracing was done, and the necessity of laying down the pencil or signalling with the other hand. On successive exposures with the first object traced the second predominates only .53 as frequently as when no tracing is done, and the images hold 20% longer on the average under the former condition.

The introspection indicates further how the tracing tends to reinforce the imagery. On single exposures *Bz* notes, "Tracing helps; the eyes go around it and the eye muscles repeat in imagery; see the images usually as enclosed spaces but with tracing I see the edge more." "Trace the triangle with the eye on the apex and it comes that way in the image." The distribution of attention in the image thus follows that of the exposure. *D* shows a similar tendency. "Image is as I draw it, e. g., imperfections in the angles I draw." *L* feels "kinaesthesis during the image, mostly in the arms." He further notes a "better control of the image when traced." *R* has a "tendency to trace the image; like eye movement, or as if the line ran around." Again, "Image comes from the tracing; sometimes forget what was shown but remember it by movement of the hand; think the tracing does not add to it except when I forget image and remember it kinaesthetically."

On simultaneous exposures the only reports of interest are those of *L*: "Try to divide attention, fixate between them and trace automatically; this causes alternation of attention between one object and the other and in imagery the figures frequently alternate."

On successive exposures *Bz* frequently reports kinaesthetic imagery in conjunction with the visual, and this kinaesthetic imagery is of the first object. He further adds, " It helps to work the kinaesthetic and visual together; the kinaesthesis brings up the visual; it fixes the outlines better; the eyes go around more on the tracing although they sometimes do so on the normal." *D* tends to " have an idea of the first object in kinaesthetic terms while looking at the second." This tendency has been noted by *D* in other series. In the present case it is more marked on the tracing series than on the normal. With *H* the second object always appears alone. She shows here and elsewhere a pronounced tendency toward immediate arousal of imagery of the object just seen,—a marked visual perseveration. *K* notes a change from a visual to a motor attitude. " Tracing often lessens interest in looks; there is a change of attitude in some figures it rules out the bad parts while others I do not like unless I trace them." *L* finds occasionally a " motor memory of the first while looking at the second,—a thought of the movements of the hand." *Ms* occasionally reports kinaesthetic imagery in conjunction with the visual and in general " likes to trace them." *R* sometimes finds that " kinaesthetic images bring up the first object." He finds that " tracing concentrates the attention."

F. *Motor Distraction*

The results of experiments on motor distraction are summarized in Table VI. The distraction was produced by writing on the table extraneous words of three or more syllables during fixation. The word was given before the object appeared on single exposures and one second before the second object appeared on successive exposures, and the subject wrote and rewrote the word during fixation. It was not possible to test the factor of motor distraction directly with simultaneous exposures because the distraction would naturally involve both objects. The best approach to the problem seemed to be to direct the motor attitude to the objects alternately and see if in the subsequent imagery the attention would follow a similar course. Tracing one object would serve as a distraction from the other and so there would be an alternating distraction and an alternating reinforcement. The experimenter said, " Right, left, right, left " during the exposure in time with the swings of a pendulum properly adjusted, starting alternately with right and left in successive trials. The subject traced on the table the object designated, and in most cases there was time to trace it only once.

The table shows no unanimous tendency on single exposures. On simultaneous exposures with alternate tracing the images

TABLE VI
MOTOR DISTRACTION

Subject	SINGLE EXPOSURE				SIMULTANEOUS EXPOSURE						SUCCESSIVE EXPOSURE		
	TIME				REPORT		TIME				REPORT	TIME	
	AROUSAL		HOLDING				AROUSAL			HOLDING		HOLDING	
					Images—alternate								
	Percentage of superiority normal	Difference / P. E.	Percentage of superiority normal	Difference / P. E.	Writing	Normal	Percentage of superiority normal	Difference / P. E.	Percentage of superiority normal	Difference / P. E.	Write / normal	Percentage of superiority normal	Difference / P. E.
Bz	7.0	1.4	.7	.7	55%	0%	3.4	4.5	46.6	6.3	.51	—41.3	9.9
Bz											.18*	—17.0	2.1
D	—6.0	1.2	—4.6	.6	100%	0%	—3.3	.3	66.0	5.8	.30*	5.2	.8
H											1.0	166.0	15.5
K											.92	60.5	6.9
Ms											.24	84.0	13.4
R	37.0	3.2	—26.0	2.9	0%	0%	—15.7	1.3	—42.1	5.3	1.23	61.7	9.1
R											.75*	9.9	1.7
T											.97	-111.0	28.0
L	—12.7	3.1	8.0	1.7	58%	0%	—10.5	1.4	—5.2	.9			
Average........	6.1	2.2	—5.5	1.5	53%	0%	—6.5	1.8	16.3	4.6	.68	24.2	9.7

* Check series; performed later.

alternate in 53% of the trials whereas normally they never alternate. They rise more slowly and hold longer under the former condition, but it is doubtful as to what is thus indicated,—reinforcement or distraction. In successive exposures with motor distraction on the second object it predominates only .68 as frequently as on the normal series and the images hold 24% longer on the normal series.

On single exposures *Bz* frequently writes automatically and keeps the attention on the figure. There is also a " tendency to name some of the figures when writing a hard word,—motor reinforcement to make it persist, and after the flash the vocal cords say ' semi-circle ' and there is more effort put forth to get the image under these conditions." *D* states that the " writing takes away from the clearness of the image; it takes away the attention." *L* finds the " spelling automatic," and speaks of the attention as " divided between work and figure." *R* also finds that " the writing is automatic and does not distract at all even if there is a visual image of the word and the hand." " It is more pleasant to write than not to; it relieves the monotony of fixation."

On the simultaneous series with alternate tracing *Bz* is conscious of eye movements. " I keep the eyes in the direction of the one last looked at; I think the attention goes with the eye muscles; effort to keep the eyes still when the flash comes." *D* notes a " feeling as if the eyes were turning." *L* gets the image in " nearly the same distribution as the object." It may be noted that in the trials in which the images alternate, with *Bz* and *L* the alternation starts with the figure last looked at, while with *D* it starts with the other, i. e., the figure presented first in the tachistoscope. There is one exception in the case of each subject.

On successive exposures *Bz* reports, " If I write carefully it has a distracting effect." Sometimes he keeps " repeating the word during the image." *D* shows two tendencies. The table shows that the writing detracts from the second object in about 70% of the trials. But in the remainder the second object is relatively much clearer than in the normal series. *D* states that the " words sometimes take away from the first although they on the whole detract from the second." He can write automatically. Further, " Normally I get an idea of the first kinaesthetically while looking at the second, and when writing I ignore this." Evidently the writing sometimes operates as a distraction from the second object, and sometimes distracts from the kinaesthetic persistence of the first during the exposure of the second. *K* shows very slight influence of the writing on the whole. He states, "Tracing the word seems to wipe out the kinaesthetic aspect of the first,—my whole kinaesthetic set as a real object; the movement may interfere with the kinaesthesis that normally develops; tracing seems to wipe out the perseveration of the first." Again, "Tracing does not seem to interfere with the image if I name the first object." Presumably these two tendencies balance one another in the results indicated in the table.

G. *Mental Distraction*

The results of the series with mental distraction are summarized in Table VII. The distraction was produced by performing continuous addition aloud, starting with one number and adding a second to it and continuing to add the second to the resulting sum. On the single exposures and on the successive with distraction on the first figure, the numbers were given about 2 seconds before the appearance of the figure,—instructions such as "17 add 24." On successive exposures with distraction on the second figure, the first number was given before the trial and the other number 1 second before the appearance of the second figure.[22] On the simultaneous method as on the series with motor distraction it was not feasible to test the variable directly. The subjects fixated the figures alternately making four alternations per exposure as directed by the experimenter's signal, "Right, left," etc.

The table shows that on single exposures without distraction the images hold 17% longer than when addition is done during fixation. On simultaneous exposures with alternate fixation the images alternate in 45% of the trials whereas they never do normally. When the distraction is given on the second figure it predominates about .82 as frequently as normally and when the distraction is given on the first figure the second predominates .15 more frequently than normally. The only large differences, however, are with *Bz* and *Ms*. With distraction on the first or second figure the images hold somewhat less than on the normal series.

On the single exposure series *Bz* finds the addition a "pronounced distraction." Consequently he finds that "when the adding is hard I want to shut my eyes for fear of making a mistake." He has been a school teacher and feels badly if a mistake is made. *D* simply says that the "addition takes

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[22] This was to enable the subject to start promptly on the addition although not starting during the first exposure as might be the case if the numbers were both given at the outset.

TABLE VII
MENTAL DISTRACTION

Subject	SINGLE EXPOSURE		SIMULTANEOUS EXPOSURE		SUCCESSIVE EXPOSURE					
	TIME		REPORT		REPORT		TIME			
	HOLDING						HOLDING		HOLDING	
			Images—alternate							
	Percentage of superiority normal	Difference P. E.	Alternate fixation	Normal	Add on second normal	Add on first normal	Percentage of inferiority add on second	Difference P. E.	Percentage of inferiority add on first	Difference P.E.
Bz	31.0	3.0	25%	0%	.29*		—9.8	2.9	—86.0	22.3
Bz					.42	1.27	17.5	2.8		
D	21.0	2.9	87%	0%						
F					.90	1.04	—29.8	9.6	15.8	5.3
G					1.02	1.23	28.8	8.0	70.3	13.8
H					1.00	1.00	125.0	24.0	34.4	13.0
K					.98	1.08	17.6	2.5	53.0	6.0
L	18.0	3.4	66%	0%	1.03	1.01	12.0	3.1	33.3	8.5
Ms					.59	1.45	25.0	6.8	53.0	11.3
R	—1.3	.6	0%	0%	1.08	1.24	62.3	9.2	44.0	7.1
R					.84*		—17.0	2.1		
T					.92	1.00	—206.0	24.8	—157.0	19.7
Average...	17.2	2.5	45%	0%	.82	1.15	4.1	8.6	6.7	11.8

* Check series; performed later.

the attention from the imagery and makes the image less clear." *L* finds the arithmetic "a great distraction, more so than the writing." Occasionally when distracted he "gives it a good look at the end." *R* says, "If I put the attention on the adding it reduces the imagery."

On simultaneous exposures the reports are almost identical with those on motor distraction (supra). *Bz* and *D* note eye movements with the alternating images, while *L* notes the distribution of attention in the image almost identical with that in the stimulus. With *Bz* and *L* the alternation starts with the figure last looked at, and with *D* it starts with the figure that was exposed first.

On successive exposures the introspection indicates that the adding in general affords a distraction. The noticeable thing, however, in Table VII is the marked results for *Bz* and *Ms* and the slight difference for the other subjects. This result might be due to one of several causes: (1) individual differences in the actual effect of attention upon imagery, i. e., whether a conscious visual impression is necessary for the arousal of the image; (2) individual differences in the method of addition,—e. g., whether visualizing the numbers interferes with the visual impression; (3) differences in the degree of attention devoted to the addition,—whether it always occupies the focus and whether the attention fluctuates.

The first of these possibilities would seem to be ruled out by the fact that both *Bz* and *Ms* report trials in which the image appears without there having been a conscious impression of the stimulus, and two other subjects do likewise. As to the second, in a series of tests upon ability in continuous addition under visual distraction *Bz* and *Ms* show a slight loss in efficiency as compared with their normal rate, but four other subjects show the same tendency. Moreover *Bz* visualizes the numbers and *Ms* does not. The third alternative remains and a survey of the introspection tends to substantiate it. Most of the subjects give occasional reports that would indicate fluctuations of attention during the addition, or addition performed with varying degrees of attention. The two crucial subjects do not show these fluctuations. *Bz* adds with a high degree of attention. He "likes to get up to 100." He hates to make a mistake in addition. It is to be noted that in the previous series on single exposures he shows the effect of the distraction the most markedly of the subjects. *Ms* finds that "when the addition went well it attached a pleasant effect to the object and when it went poorly it took the attention from the object." As a matter of fact it seldom went well and his general opinion at the end of the series was that he "hated it." It is probable that with these two subjects the mental state while observing the object and doing mental addition approaches more nearly and constantly to complete distraction from the object.

Thus the fact of the influence of attention upon imagery which has been more or less manifest throughout the entire investigation receives definite corroboration. Although a complete control of attention is impossible, the results give rather strong indications of the importance, for imagery, of attention bestowed upon the stimulus.

IV. Summary

A. *General Tendencies*

The preceding experiments have investigated the rôle in imagery of complexity of contour, size, length of exposure, interest, motor reinforcement, motor and mental distraction. Each of these variables was applied to a stimulus object and the effect upon subsequent imagery noted. Three methods were used in each case,—single, simultaneous and successive exposures,—with fairly consistent results. The first five variables mentioned tend quite generally to reinforce visual imagery and facilitate its arousal, and the last two have the opposite effect. If the general averages at the bottom of the columns in Tables I-VII are considered it is evident that the smallest differences are shown by all three methods for size and length of exposure. Motor reinforcement yields a marked effect by all methods, and interest also ranks quite high. The effect of complexity and motor distraction is considerable in simultaneous and successive, and that of mental distraction in single and simultaneous exposures.

B. *Individual Differences*

Average results must not be allowed to obscure the individual differences, for while the subjects nearly always show the influence of the factors in question, there are great differences in degree. Furthermore the introspection reveals that the given factors operate by different means with the different individuals.

If the results for the subjects who participated in the experiment on a number of variables are followed through Tables I-VII, it is evident that individuals vary as to the factor that most affects them. For instance, *Bz* seems most affected by motor reinforcement and least by size. *D* is most susceptible to motor reinforcement and least to lengthening of exposure *L's* imagery is most facilitated by interest and least by lengthening of exposure. *R* is most influenced by complexity of contour and least by distraction.

In the course of the work a number of individual tendencies have been brought out. The salient points for each subject may be briefly mentioned.

Br notes the presence of kinaesthetic factors during the presentation of the stimulus, which apparently facilitates the subsequent imagery.

Bz works kinaesthetic imagery to aid the visual and in addition to central kinaesthesis actual eye movements are frequently noted. The distribution of attention during the stimulus is a great factor in influencing imagery. If one part is fixated that part becomes clearer in imagery.

C has a tendency to accentuate attributes of the stimulus such as size.

D on successive exposures has a tendency toward a persistence of the first object in kinaesthetic terms during the exposure of the second. Any distraction during the second or a shortening of its exposure tends to interfere with the persistence of the first and to detract from its subsequent imagery. Much depends too upon distribution of attention during the stimulus period.

F shows considerable play of associations with simple figures and this correlates in general with reproductivity of the figures in imagery. It is the pleasant associations that have the greatest effect.

H's most marked feature is a pronounced visual perseveration. In single and simultaneous exposures the images usually appear very quickly and exactly reproducing the stimulus object. In successive exposures the second object appears alone in imagery under all conditions, and rises quickly.

K shows like *D* a kinaesthetic persistence of the first object during the second on successive exposures. Distraction on the second militates against imagery of the first.

L reports far more kinaesthesis than any of the other subjects. The visual image is frequently filled in with kinaesthesis. In the voluntary arousal of a visual image with merely a word stimulus, he experiences great difficulty in getting a visual image at all. Kinaesthetic imagery is sometimes used to mediate the visual. On successive exposures there is frequently a motor memory of the first object during the second. The distribution of attention in the imagery follows that in the stimulus. In some cases an attribute of an object appears in imagery although the object itself does not do so.

Ms has a great play of associations. The objects tend to become real in imagery with a very definite and complex context. This tendency correlates with reproductivity to a considerable extent. He also occasionally notes kinaesthetic imagery in conjunction with the visual.

Mt has kinaesthesis frequently in connection with the visual imagery. She feels it would be easier to draw the figure than visualize it, and there is a tendency to draw the part that is interesting.

R, after tracing an object, often recalls it for visual imagery by an actual movement of the hand. In many other instances he names the object as an aid in recalling it.

C. *Conclusions*

1. Complexity of contour of a plane figure, increase of its size, lengthening of its exposure, interest aroused in it and motor reinforcement by tracing its outlines during fixation tend, in general, to facilitate the arousal of visual imagery of that figure. Distraction by performing mental arithmetic or by writing extraneous words during fixation has the opposite effect. The results are, on the whole, most marked with motor reinforcement and with interest, and least marked with increase of size and lengthening of exposure.

2. Individuals differ in their susceptibility to the above factors. The visual imagery of some is most influenced by motor reinforcement; of others by interest; and of others by complexity of contour.

3. Individuals differ further in the means by which these factors operate. The principal points of difference are:

A. Other imagery,—notably kinaesthetic,—is with some individuals involuntarily employed to reinforce or mediate the visual. There may be kinaesthetic imagery of the movement of the hand or eyes around the contour of the object, or verbal imagery of its name.

B. Some individuals have a kinaesthetic persistence of a visual stimulus immediately after its disappearance, and interference with this detracts from subsequent visual imagery of the object.

C. Distribution of attention during the stimulus period conditions with some individuals the distribution of attention during the subsequent imagery.

4. It seems probable that the factors studied in the present experiment are somewhat interrelated and converge toward

a single factor,—the directing of attention to the stimulus object. The introspection indicates that the effect of complexity of contour and the effect of size are due primarily to motor elements. The motor aspect, in turn, is described as producing a greater concentration of attention. Interesting material is reported as arousing associations and stimulating attention to a greater degree. When the attention factor is made the subject of specific study by means of distraction during fixation of visual stimuli, the results show that, although in certain sporadic cases the arousal of imagery of the stimulus may proceed entirely involuntarily, a higher degree of attention devoted to an object generally facilitates the arousal of imagery of that object.

5. Apart from the question of "visual type," there are individuals who can reproduce a visual image immediately, and others for whom the visual is mediated or reinforced by imagery (or in some cases sensation) of another mode,—notably kinaesthetic. Grouping of individuals on the basis of such a tendency shows no correlation with a grouping on the basis of clearness of persistence of imagery.

A STUDY IN CORRELATION OF NORMAL COMPLEXES BY MEANS OF THE ASSOCIATION METHOD

By Lucile Dooley, University of Tennessee

I. This experiment in the Association Method was undertaken with a view to making a study of emotional complexes. More especially it was intended as a test of the validity of the Freudian theories in regard to fundamental complexes, if such a test could be made by means of the Word-Association Method. Through the work of Jung, and some others, the Association Method has become established as a means of "tapping the subconscious" in cases of abnormal mental states or of nervous disease. Extensive experiments have been made with normal persons also, but not, the writer believes, with the specific aim of testing for the presence of what may be called the "Freudian Complexes." To see if the Association Method could be used to uncover such complexes in normal persons was, then, one aim of the experiment. This sort of experiment has been used effectually for this purpose in the testing of hysterical or neurotic cases, in which the symptoms of complex are pronounced. While the "complex signs" in the normal individual are seldom so clear and unmistakeable it was thought that if the complexes that are emphasized by Freud were present in any degree the results of the experiment should yield some traces of them.

Incidentally the experiment was expected to yield some new data for the study of individual types, since there are still many problems along this line inviting further investigation.

II. The method pursued differed from the auditory-verbal method commonly used in that the stimulus was a printed word exposed upon a card-changer and the response that marked the reaction time was a motor response made by pressing a Morse key with the finger. This method has some advantages over the auditory-verbal method, and also some inherent disadvantages that became apparent in the course of the experiment. Exposing the printed word gives less variableness in the presentation of the stimulus than does pronouncing the word, with fewer chances of misinterpretation;

also, it allows investigation of the very interesting phenomenon of misreading. In the similar phenomenon of mishearing, occurring with the auditory stimulus, it is not so easy to determine how far the error was due to external causes and how far to psychical causes as it is in misreading. With the printed word and the card changer the method is more mechanized. Besides, this method permits the use of the chronoscope which is a more accurate recorder of time than the stop watch. The method has obvious disadvantages when uneducated observers are used, but the writer can see none that would operate with educated observers. Another point in favor of the printed word is that people are more accustomed to seeing isolated words than to hearing them, and that, therefore, the experiment is given a less unnatural atmosphere by the use of the visual stimulus. The method of the reaction is more open to criticism, as will be shown later.

The stimulus words formed a series of 154, with Jung's list of 100 taken as a basis. The number was increased considerably above the usual 100 in order that there might be at least 100 perfect reactions from each observer after all erroneous and interrupted ones were subtracted. The words were mostly substantives and almost all were of possible significance, it being thought that words significant to one observer would be neutral to another and that the proportion of significant and neutral words would adjust itself in each case. This supposition apparently was justified by the results. The words were printed in uniform type—spaced capitals—and exposed on a card changer that was connected with a Hipp chronoscope, so that the fall of the shutter, exposing the word, started the clock. The reaction was made by hand, with a Morse key, which stopped the clock. The association word was uttered at the instant that the motor reaction took place. A voice key was at first tried, with the purpose of making the word uttered by the reagent act upon the clock. This could not be made to work satisfactorily, however, so the Morse key was substituted. Eight observers, five women and three men, seven of them being students and instructors at the University, and presumably normal persons, participated in the experiment. The eighth was a distinctly abnormal mental type, being a young woman suffering from an hysterical neurosis. Before the main experiment was begun all observers were given a practice series of from 25 to 50 words so that none was inexperienced.

As the "Ausfrage" method was used, involving full introspections with taking of notes by the experimenter the experiment with each individual necessarily covered many hours. With only two exceptions the work was never carried on for more than fifty minutes with a single observer on one day. The usual method followed was to have each observer for one hour each week, and as a minimum of 12 words and a maximum of 30 could be given and reacted to, with noting down of the introspection, in the hour, the experiment extended over a period of nearly four months, with all but the observers above mentioned. These two performed the experiment in three and five days, giving two or three hours at a time to the work. Fatigue effects were especially looked for in these cases, and, while present, were not found prominent.

After the series had been completed it was repeated with such observers as had time for the repetition, in order to test the evidences of complexes. Only two observers were able to complete the repetition series, while two others repeated about one third of it. The other four had no repetition. This series is therefore not treated separately, but its results are considered, along with the results of the main series, in the part of the thesis dealing with the occurrence of complexes.

There is a distinct advantage to be claimed for that feature of the method here employed that causes the experiment with each individual to extend over months instead of over one or two hours, as in Jung's method. This is the eliminating of the possibility that the association series will be dominated by a single mood, or affected by a single unfavorable condition. It is true that the conditions for each hour cannot be uniform but of more advantage than uniformity is the certainty that almost the whole range of the personality will come into play. As conditions were carefully noted at each period the effect that unusual circumstances were likely to have could be better measured when many periods of an hour each were included in each series.

As five out of the eight observers were of the visual and kinaesthetic types, associating a visual or kinaesthetic image before a word, and claiming great difficulty in getting a word as a response at all, even in the most trivial associations, these persons were permitted to give the motor reaction only, and the instructions were varied to read "As soon as another word, or a definite idea or image, comes to mind, react by pressing the key." Such a variation might be suspected of affecting the reaction time to such an extent as to make it of no experimental value. There is complete lack of evidence that it had any material effect, however, while as evidence that it did not cause any important variation in the time there is the fact that observers of this type are at both extremes of the scale of quickness of reaction, and that their reactions are susceptible to the same physical and psychical influences, show the same fluctuations, the same relation to emotions, and can be correlated in the same manner, as the verbal reactions. There is also the fact that some of these observers finally learned to react with words and after this was fully learned as a mode of reaction the time averaged about the same as the purely imaginal reaction time. The word was nearly always a mere adjunct to the image, except where there was a clang-association, and in such cases the image connected now with the stimulus word and again with the reaction word in almost regular alternation. Clang-associations were wholly confined to the imaginal types, indicating that the verbal reaction was unnatural and forced. An advantage may therefore be claimed by the wordless reaction in such cases; namely, that the association obtained would be of the sort most natural to the observer, and would be accompanied by a minimum of difficulty. It seems probable that by this method there would be a smaller mean variation than by the verbal method, as the motor reaction tends to take place, after a certain interval, regardless of the content of consciousness, whereas, if there has to be a spoken word, there might be numerous difficulties in the way of its articulation. As a matter of fact there is no significant difference in the mean variations of the different observers, leaving out the one abnormal case. What difference there is is on the opposite side; i. e., the imaginal types show a slightly greater mean variation than the verbal types. Individual differences, to be brought out later, seem the most probable cause of this.

While it is felt that this departure in method does not invalidate the results of the experiment, it does raise some real difficulties. Because the tendency toward the motor reaction tends to fulfill itself regardless of the content of consciousness the delays caused by distraction or emotion are slighter than they might be if words were demanded. The fact that those observers who did react with words showed no greater variation in the face of distraction might well be explainable by differences of individual type. The doubt that arises from this condition of affairs affects the validity of comparisons between these two types. When the results of this experiment are compared with results obtained by Jung, Riklin, Wells, Kent, and others, moreover, the lengthened reaction times here considered significant are so much less lengthened than those of the above-named experimenters that they may seem insignificant. Rarely does the time, in this experiment exceed 3.5 seconds, and 2+ is the commoner maximum. Nevertheless, within each individual series there is a sufficiently typical variation, though its range is small. While the time does not furnish so striking an indication of the complex as when time is more prolonged, it nevertheless furnishes a sign, since it is generally relatively longer in cases where there is reason to suspect an inhibition than in others. By this method there is left room for falsification by the observer, of course, but the reaction time, bodily movements, and general behavior, supply some check to falsification. It should be said that all observers gave the *appearance* of good faith, though of course this cannot be trusted too far. The experimenter recognizes the fact that falsification may be unintentional or unconscious.

When the experiment was about two thirds completed the draw-backs indicated above became apparent, and all observers were instructed to react verbally, no matter how much effort it cost. Some had completed the series at this time and were beginning the repetition. Some, however, had nearly completed the series and could not spare time for the repetition. There was only one from whom no verbal reactions were obtained, while others of the imaginal type gave anywhere from one tenth to one half verbal responses. This irregularity makes difficulties in tabulation of results, but the difficulties are not insurmountable. While many points that might be of interest, such as would arise from comparisons of different verbal forms, cannot be treated because of the irregularity and dissimilarity of the reactions, there is no good ground for thinking that the main objects of this investigation are rendered inaccessible.

III. The first step in correlating the results of the experiment was the determination of the association type of each observer. The preliminary experiment had shown that certain types of mental reaction were associated with high and low reaction time. A study of the typical reactions revealed the mental character of the subject.[1] Because of the greater number of emotional reactions in the main series this correlation is less definite than in the preliminary series, where no words designed to arouse complexes or emotions were found. Still, a relation between the average reaction time and the mental type is discernible. This study of reaction-type neces-

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[1] Reported before Southern Society for Psychology and Philosophy at Atlanta, December, 1913. See "Proceedings of Southern Society," etc., reprinted in *Psychological Bulletin*, February, 1914.

TABLE I

Correlation between time and association type

Observer	No. of reactions counted in averages	Av. seconds	Median	M. V.	Formal Type			Reaction Type	Content Type				General Type
					Co-ordinate	Sub-ordinate	Supra-ordinate		Emotional reaction	Ego-centric	Common	Particular	
*R.	144	*.647	*.577	.210	17 Av. time .827	127 Av. time .754	0	Vis. Im., clear cut, few details or additional tendencies not verbal except when forced.	16⅔% .831	10% .663	13⅓% .876	86⅔% .651	Concrete, personal reference, occasionally emotional, non-self-conscious, few complex signs.
H.	136	.871	.830	.227	16 Av. time .864	119 Av. time .921	0	Vis. Im. and clang words, few details, but additional associations.	27⅓% 1.164	40% .828	5% 1.830	95% .874	Concrete, intellectual repressed emotion, personal reference, unconscious of comp. signs.
Be.	143	1.021	.925	.344	19 Av. time .748	107 Av. time 1.173	17 Av. time .929	All sorts of imagery, kinaesthetic predominant. Words used as names for images.	35% 1.462	8% .923	88⅔% 1.01	11⅓% .925	Concrete, aesthetic, emotion, richly imaginal. Complex mildly pleasurable.
K.	150	1.05	1.013	.249	75 Av. time 1.047	72 Av. time 1.212	6 Av. time .926	Verbal, automatic, few images, few additional associations.	17% 1.316	4⅓% 1.474	99.4% 1.014	.6% .738	Abstract, intellectual, repressed emotion, many complex signs which are unconscious.
S.	150	1.089	1.037	.266	0	150 Av. time 1.089	0	Vis. Im., clear and detailed, no words, meanings.	24% 1.717	7⅓% 1.085	73% 1.048	27% 1.126	Concrete, emotional, non-self-conscious, complex signs conscious.
O.	142	1.14	1.076	.274	116 Av. time 1.136	16 Av. time 1.528	1 Av. time .865	Verbal, automatic, with frequent thought processes.	20% 1.446	2.6% 1.511	97% 1.028	3% 1.424	Intellectual, somewhat emotional, logical, complex signs conscious.
Ba.	150	1.363	1.2	.382	6 Av. time 1.384	135 Av. time 1.472	0	Vis. images, detailed and definite. Words only names, meanings.	23% 2.647	39% 1.364	18% 1.451	82% 1.201	Concrete, self-conscious, moderately emotional, complex signs, often conscious.
T. Abnormal	136	1.788	1.612	.580	105 Av. time 1.012	31 Av. time 2.012	1 Av. time .515	Verbal, with an effort, frequent visual and auditory images, frequent affective tone.	39% 2.816	10% 1.743	70% 1.767	30% 1.923	Concrete, emotional, slightly self-conscious, many complex signs which are conscious.

* R. is suspected of reacting prematurely, making time too short.

sarily precedes an examination for complexes, as the manner of exposing a complex is obviously not independent of the observer's natural association habit.

The correlation between the average and median reaction times, and the reaction type of the observer is brought out by Table I. The commonest type of reaction is by visual image, with the association word as a mere adjunct, oftenest a name given to the image, when it is not omitted entirely. Only two, K and O, responded easily and naturally with words. The others did so with effort, often gave clang associations, or variations of or additions to the stimulus word, and claimed that the image was always first in order and that it was the essential part of the association. No generalization can be made as to the relative quickness of the verbal versus the imaginal type. The imaginal types stand at the beginning and at the end of the scale,[2] while the verbal automatic types are fourth and sixth in order.

The observer holding fourth place had very meagre associations, introspection frequently showing nothing beyond the word. Perseveration was of fairly frequent occurrence, and evidences of unconscious inhibition were present. The observer in sixth place had frequent thought processes though few images. This observer was cognizant of the purpose of the experiment, therefore his associations cannot be placed altogether on a footing with the others. Allowance is made for this fact in the analysis of complexes—but this knowledge on the part of the observer has less effect than might be expected. As a rule any thought of connection with the experiment was a *secondary* association, while the first was, like those of other observers, a naïve response. The main effect of this foreknowledge is seen in the reaction time. The attitude was one of expecting "significant" words, and this often prolonged the time when no emotional complex was present. Other vitiating factors have been noted, as far as possible, in all cases, and any reactions that were suspected of being affected by fatigue or physical disposition are excluded from the tabulation. The reactions of one observer which took place on a very warm afternoon have all unusually long reaction time, so these are excluded from general tabulation, though they are not altogether disregarded. Whenever conditions were unusual the results were scrutinized carefully and compared with those obtained under normal conditions and where differences were apparent the former class of results were not accepted as of full value, though they still might be useful for determining some points. Whatever examples of associations are given, and whatever conclusions are drawn, in this thesis, are taken from those results only which are free, as well as the experimenter can judge, from vitiating influences.

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[2] Observer R, who stands first in Table II, as the observer who has the lowest average time, gives some reason for suspecting that his reaction time is in reality too low. He may have reacted before an association came. His quantitative data will be starred wherever it occurs, in order to show that it is questionable. In other than quantitative aspects his reactions are entirely valid, and will be treated as valid.

Though no direct correlation is apparent between the imaginal and verbal associations and the reaction time, there is a correlation between the time and the *richness* of the image, or the profusion of images. Those of imaginal type who had images with few details, and with no personal meaning attached, have a low reaction time. Those who get clear cut and detailed images, with personal associations or frequent affective tones, have a high reaction time. Associations in a subordinate or predicate relationship to the stimulus predominate in the imaginal type, while co-ordinates predominate in the verbal type. Supra-ordinates are of rare occurrence in both. So far as the experimenter knows this correlation has not been observed in any previous work with the association method. How far it holds good must be determined by experiment with a larger number of individuals. The subordinate, or predicate, type, indicates, in the cases here studied, a concrete type of thought, while the co-ordinate type is rather abstract. The latter has a shorter reaction time than the former, when emotional and significant reactions are excluded.

There is some relation apparent between the egocentric type of association and the time, as contrasted with the less personal type. With the exception of observer H, who has a very low reaction time, a large number of egocentric reactions accompanies a high average reaction time. The introspections reveal the degree of *self-consciousness* also, and this bears a direct relation to the reaction time. Observers Ba, K, H, and T, are usually self-conscious, while the other four are less so. Observers Ba and H frequently saw themselves in the picture brought up in an association.

In general it may be said that a low reaction time characterizes common, non-self-conscious associations and abstract types of thought. This is true when " complex " reactions are excluded. High reaction time characterizes the egocentric, self-conscious, and concrete association types. When emotional reactions are ruled out this correlation becomes definite.

As in most experiments of this sort, individual differences cause many exceptions to be made to almost every general conclusion that one may attempt to draw. Nor are the individual traits revealed the least interesting and important results yielded by such an experiment. The discovering of the individual character by means of the experiment seems in itself worth while, and suggestive of practical application of the method.

A most striking and important difference, and one that has been fully treated by Jung (10, 11), is the difference between the normal and the abnormal observer. One abnormal observer, a young woman suffering from an hysterical neurosis, was included in this experiment for the purpose of making the comparison at first hand. The greater part of the published material on complexes deals with abnormal cases, and the experimenter felt the need of getting experimental evidence on her own account for the difference—or rather the exaggerations—of the abnormal type, when compared with the normal, in order to avoid misconceptions. The abnormal observer was a typical neurotic, not an extreme case. The abnormal *exaggeration* of various association phenomena is in her case fully evident. She shows the highest average time, and also the highest single reaction time. Her average time is 1.788, which is more than 400 sigma in excess of the next highest. Her highest single reaction time is 8.487, while the nearest approach to this by any other observer is 4.958. Five of the normal observers never exceed 2.880 as a maximum. This observer frequently showed difficulty in obtaining an association even when no complex was in evidence. When a complex was struck emotion was much more intense than in any of the normal observers though some of the latter showed a greater number of emotional reactions. The abnormal observer showed frequent inhibition and " blankness " followed sometimes by a sense of complete helplessness and despair. This was once strikingly described as " a feeling as if I were in a burning theatre and could not get out." The feeling of helplessness grew until she finally reacted (by pressing the key) in order to end it. The neurotic patient exhibits the phenomena of inhibition and emotion in much more marked degree than any of the other observers. Infantile associations were more frequent and more important in this case than with the normal observers, with whom, indeed, they could seldom be detected. The principal differences between the normal and the abnormal types are shown in Table II.

TABLE II

OBSERVERS	NORMAL							ABNORMAL
	Ba	Be	H	K	O	R*	S	T
Number of infantile associations	4	2	5	0	0	5	5	9
Number of marked inhibitions	11	4	4	6	16	8	9	16
Number of cases of painful effort	6	1	3	1	3	2	6	13
Number of cases of marked emotion	6	6	3	5	8	7	9	18
Number of failures to associate	6	0	1	0	7	4	4	11
Number of failures to make reaction	0	0	0	0	1	2	0	5
Highest reaction time	4.958	2.880	2.014	2.23	2.5	1.958	3.2	8.487
Lowest reaction time	.572	.338	.331	.469	.559	*.347	.397	.582
Mean variation	.382	.344	.227	.249	.274	.210	.265	.580

* This marks the observer whose reaction time is suspected of being too low.

A difference that cannot fail to be important to an investigation with the end in view of studying complexes is the sex difference. The average of the median times of the men observers and of the women observers is exactly the same, .951, if the neurotic patient is omitted from the woman's list. Again, here, the number is too small to permit of generalizations, but it may be said that were it not for the average of one man who gives some reason to suspect that his association time is too short—that he reacted before an association really came— the average of the men would be larger than that of the women. One male observer who was unable to complete the experiment, and whose results are therefore not included, was among the long-reaction-period types, and if his results had been tabulated with the others the reaction average for men would have been noticeably higher than that of the women; the number of normal observers of each sex would have been equal also, as was at first intended. Other experimenters who have published results have almost uniformly found the time of women to be longer than that of men, but Wells (19) pertinently suggests that this result was very likely influenced by the fact that the experimenter was a man, which circumstance might give rise to many more inhibitions in the women. In this experiment the experimenter was a woman, which may have had an influence in raising the average of the men. The mean variation of the men is greater than that of the women, which fact is in line with the foregoing supposition, though it would be unjustifiable to assign the circumstance of the experimenter's sex as the cause of this.

The sex differences found may be summed up as follows: Median time for each .951, but with reason to believe that the men's median should have been higher; mean variation for men, .289, for women, .271; egocentric reactions for men, 17.2%, for women, 14.9%. Men have 42% common associations, 58% particular, 20% emotional, 26⅔% "significant," 7.7% inhibited, 64% predicate, 25.9% co-ordinate, and .06% supra-ordinate. Women have 66% common associations, 34% particular, 25% emotional, 23% significant, 3.8% inhibited, 74% predicate, 18% co-ordinate, 3⅓% supra-ordinate.

Another factor that is expected to play a part in determining individual differences is that of age. As age increases repressions should increase, associations become less personal and more standardized or conventionalized, and complexes become more clearly marked. But here again individual variations of character make it difficult to generalize. Perhaps *mental* age, or better, degree of sophistication, if these could be determined would furnish better criteria. The ages ranged from 22 to 38, and as the years of the twenties and thirties are apt to be fullest of emotional experience there should be room here for a distinct progression. There is such a progression although it does not proceed with regularity. Cultural differences are more apparent, and these modify the age differences, but they cannot be tabulated in the same way, for obvious reasons. It is found that the younger give, as a rule, more egocentric reactions but that the middle grade have more significant and emotional associations, excepting the abnormal subject, who was among the younger. The older ones have rich and profuse associations, with less personal reference.

Those who have the habit of imaging naturally have great difficulty with words that do not suggest an association capable of being imaged. Abstract and emotional words may thus be met by a delayed reaction which has a cause quite outside the ideational content of the word. The verbalizers have no such difficulty and may respond to abstract words with greater ease than to concrete and simple ones to which they cannot readily find an opposite or synonym. This point also brings out the fact mentioned by Wells (19), that few " free " associations are really free when in an experimental series. They tend to become "controlled," that is, the observer sets up a certain form of response for himself to which he unconsciously tries to conform. In determining the ' complex ' value of a reaction these peculiarities of individuals must be taken into account. The verbalizers also have trouble with a reaction word of two or more syllables, and with one that is a different part of speech from the stimulus word—in short, with any response that does not fit into their customary method. When the imaginal type gives words he feels none of these difficulties because the words mean much less to him. The egocentric type likewise hesitates over a word that does not recall a personal experience. There is a sort of perseveration of the *mode* of response that is not altogether dependent upon the observer's mental character. It depends partly upon the mode with which he starts out. The experimenter may be able to change this mode by suggestion, and the new mode thus set up soon becomes habitual.

The obstacles to getting exact correlations and drawing definite conclusions that have been mentioned are probably present in the most extensive and carefully performed experiments almost as fully as in this attempt. The arbitrary uniformity of the response would not really obviate the difficulties involved here in the discrepancies between imaginal and verbal types. The writer is inclined to think that the fundamental differences in these two types have not yet received discriminating consideration. Those who were imaginal in their thinking would have the same difficulty in finding a verbal response that the observers in this experiment had, and the result would be a prolonged reaction time because they are allowed no alternative to a verbal reaction. On the whole, making allowance for difference in method, the results found here as to mental types, age, and sex, and as to the rôle of external influences, do not differ materially from the well known conclusions of Jung and Riklin, Wreschner, Ziehen, Wells, and others. That a great part is played by emotion is axiomatic. With Ziehen (22) we find that unpleasant emotion has the greatest retarding effect, while pleasurable emotion so facilitates the response as to bring the time below the average. Aschaffenburg (1) has found that fatigue and distraction increase the tendency to clang, irrelevant, and perseverating associations, and this experiment corroborates his assertion. Lipmann (14) maintains that this fact is not a disadvantage in investigating complexes, as it actually means that repression is lessened, that inhibiting power is lowered, and that the complexes have a freer outlet. The data of this experiment are not without confirmatory evidence for this assertion. Still, in analyzing the experimental data with the purpose of discovering the emotional complexes that may be present, all such factors as fatigue, distraction, interruption from outside, and the physical conditions, should be given full weight as interferences. The central fact that makes the association method of value is that the observer reacts toward the word in some measure as he would react toward the actual situation that the word represents. The extent to which he actually does identify the mere word with a situation is often surprising to one not initiated into the mysteries of the association experiment. To some word of disagreeable import, for example, he reacts quickly, shuddering, without waiting to get a definite association word " because he wanted to get it over with and thought the reaction would end it," which is, perhaps, equivalent to turning tail and taking flight. It is this attitude toward the words used in the experiment, which quickly establishes itself in every observer, that causes the centrally aroused processes to be generally stronger than any external interferences.

IV. Having cleared the ground by the foregoing discussion of types, differences, and general reaction-characteristics, we come to the main part of our inquiry, namely, How are complexes indicated by the reactions collected? There are complexes present, as has been hinted already. How these are determined, what evidence for them is accepted, and what is the general significance of those found, also what their particular significance for the Freudian Theories, will occupy the remainder of the thesis. No complex is catalogued that is not indicated by more than one ' complex sign,' and as a rule more than one reaction will be required to establish a single complex. The ' complex signs ' considered as indicative of complex are here enumerated and described.

1. The quantitative indication of unduly prolonged reaction time is the first to be noted. Jung states that the excess must be at least .5 seconds in order to be significant. His average reaction time is 1.5. Some experimenters say that the smallest increase to be considered is 1 second. It depends upon the average or normal reaction time of the reagent. As has been pointed out in the discussion of the method, the reaction time is shorter when the stimulus is presented to the eye and the effective response is made by hand, with an instrument of such accuracy as the chronoscope. than when the stimulus is

auditory, the response oral, and when the stop-watch is used, which instrument involves the experimenter's reaction time as well as the observer's. The gross average of all the observers in this experiment, excluding that one who was suspected of reacting too quickly, and also excluding the abnormal subject, is 1.013. It seems fair, then, to count any increase exceeding .3 seconds as possibly significant, provided that there are other supporting evidences, that is, some of the other 'complex signs' listed below. On the other hand, the striking of a complex is not invariably signalized by lengthened reaction time. Pleasurable emotion may quicken instead of delay. Likewise, unless there is an effort, conscious or unconscious, at repression, the time may not be very much lengthened. The prolonged reaction time, then, cannot be taken as the invariable sign of a complex. It is only one among others, and may be lacking. A very short reaction time may be as truly a sign of complex as a very long one. One observer responded to *dream* by instantly pressing the key,—time .206!—, with no consciousness of her mistake, but the significant introspection, "I thought, 'Other people dream, but I don't.'" Such shortened times as this are not included in the tabulation, but they cannot be overlooked as complex indicators. Even when an association is given, and no error can be detected in any part of the response, the time may be very short. An observer who had an obvious "In love" complex responded to *bride* with a time of .363, and with the trivial associate *groom*. This, however, was in the repetition series. When *bride* occurred in the first series the response was signalized by length of time and by a significant association. It is not unusual for the response to go to the opposite extreme when reproduction is tried, the first response having been significant. Jung (10) has observed that the subject is seldom able to reproduce a "complex" association.

2. Perseveration. Sometimes there is no apparent connection between the stimulus word and the reaction word, yet the latter is used, not once only, but several times; also, it is used neither consecutively nor at regular intervals. This is perseveration, and if it is well marked, if the word is usually irrelevant, if the stimulus words are of a significant sort, and if the reaction time is significant in any way, it may generally be taken as a sign of complex. An example is the response *hope,* given to the following stimulus words, friendship, master, pity, wealth, sin, victory. In each instance there was another complex sign present, sometimes more than one. If the perseverated word itself

occurs among the stimuli it is pretty sure to bring a significant response. The exact interpretation of this would depend somewhat on *where* the word occurred, whether before or after the perseveration began.

3. Vacuum, or blankness of mind. This, of course, cannot always be taken as a complex sign. It may occur if the word is difficult to understand, if it seems remote to the observer, or if it follows a word of a very different import (although, as a rule, unless some unusual reaction has just taken place, words are not affected by preceding words, and as a consequence of the method here employed there is plenty of time for the effect to die out before the new one comes). Inattention, and distractions from the outside may also induce blankness, as may fatigue. If none of these factors are present, and if there is some other complex sign as well, vacuum may be taken as an indication of complex. Example: *part*, time 2.870, introspection—"Could think of nothing for a long time. Finally a vague image of people taking sides, seemed to be women, maybe suffragettes." This, when analyzed, was found to be related to one of the dominant complexes of the subject.

4. Irrelevant, or unmeaning response. This may take two forms, (a) the qualitative relations between a stimulus and a response are of unmeaning character, (b) the response itself is without meaning—i. e, clang associations. The second form is less common than the first. The introspection sometimes reveals a relevance that would not otherwise be apparent, even to the observer himself. An example of the first form is *book—tree* (perseverative). An example of the second is *dear—mear.*

5. The phenomenon of misreading must be taken as significant if there is supporting evidence of complex, or if no cause is to be found in the printing of the word, the lighting, etc. Thus one observer, after having used *tree* as a perseverative response five or six times, misread *tree* as *thee,* and associated *me,* when *tree* occurred as a stimulus word; as the word was clearly printed and as no other observer had any difficulty with it, save one, who also presented evidence that *tree* touched a complex, this case of misreading is interpreted as a complex sign. Analogous to *mis*reading is inability to read. One subject had great difficulty in reading *marriage* (a word reacted to significantly in every case) and said that the letters seemed to be one above the other and jumbled together. The word was, of course, clearly printed.

6. The failure to get an associate will usually occur with No. 3, "vacuum," but not always. Sometimes there is a

rush of ideas but nothing articulate. Of course this failure may be due to some of the causes mentioned in the discussion of individual differences and reaction-habits, or it may be due to the strangeness of the word. There are no really uncommon words in the list, however. We might venture to say that every one of them would be met with in the reading of a single issue of a daily newspaper. When no "harmless" explanation will satisfy the case, then the failure may be taken as an indication of complex. The observer's own account of the reason for the failure is by no means trustworthy, even though offered with full sincerity. An unmistakable case was the word *abuse*, when no word came, and no idea of any sort was fully conscious until the observer finally pressed the key, fearing lest the weight on the clock should reach the bottom, while she remained passive, and so jar the mechanism. It was therefore not a true reaction, and the time is not so long as it would have been had she waited for an association. After the reaction came a picture of a horrible looking man, who was thought to be *abusing* someone. The time elapsing before the clock was stopped was 8.487. This observer had an experience in childhood with which this image was remotely connected. She had now an exaggerated dislike, amounting almost to hatred, of men, as *masculine*, as part of a strong sex-complex.

7. Failure to give the motor reaction. In some few cases the association is so absorbing, because of its strong interest or its affective tone, that the motor reaction is inhibited or, what amounts to the same thing, forgotten. Such instances are very rare, and this infrequency of occurrence gives them the more importance. The *habit* of response by the motor reaction is usually so firmly fixed that it tends to take place after a certain interval, even though no associate has been obtained and though the mind remains a blank, and all other activity is inhibited. This tendency, by the way, has much to do with the fact that there are relatively few very long reaction times. When this habitual action is inhibited we may be pretty sure that there is a strong reason. One such case is in response to the word *knight*. The preceding word was *lady*, and this had recalled a romantic picture, from a fairy tale, that related to a very common complex, exposed by other words as well. When *knight* came the observer exclaimed enthusiastically, "Ah! the Prince! the Prince! the Prince!" and quite forgot to press the key. She explained that "she was so delighted that the prince came in just after

the lady." Such excess of feeling, however, must have meant a reinforcement of the association, due to a complex.

8. What might be a variation of the failure to associate is the *surrogate response.* The observer cannot find an association word, is conscious that the word likely to come is one he would wish to suppress, perhaps, so glances around, and names some object in the room, or, possibly, something connected with the dress or the person of the experimenter. Since the giving of objects near at hand is very unusual, the associative thought nearly always running in paths remote from the present time and place, such instances are likely to have a significance.

9. Embarrassment, evidenced by a nervous laugh, or by much hesitation and stammering, is a universally recognized sign. A good many instances of this occur in these experiments, sometimes with words that are obviously significant, such as *marriage,* sometimes with words that seem trivial and which are responded to in an unusual manner by only one observer. An instance of the latter sort was *key,* with no reaction word, but a visual image of a big brass key, associated with a brown door, then complete blankness of mind. The reaction was accompanied by a laugh and a slightly flustered manner.

10. Conscious emotion is not always a sign of complex, and on the other hand, a complex may be exposed without any accompaniment of conscious emotion. Cases of emotional reaction are numerous, as some of the tables given in the earlier divisions of the thesis show. They are more likely to be complex indicators when the observer is unable to account for the emotion. A mere sense of pleasure, or of aversion, detached and impersonal, is not necessarily, or often, a complex sign, or so the correlation of these results indicates. One example where emotion was strong, where a complex was undoubtedly present, and where the subject fully understood her emotion is found in the association *child—mother,* with a reaction time of 1.752. The introspection contains this statement, " I had a feeling that I can't quite express when I saw the word *child.* A feeling that I always have when I see a little child—half pleasant, and yet it hurts, so not pleasant in a way. A feeling as if one would like to cry." This complex had been analyzed before, and the repression was largely freed. This might account for the fact that the time was not very long.

11. The association word may be the chief indicator of the complex. It may be related to the stimulus word in a way

that reveals a significant background or it may indicate an inhibition or a state that is *almost* a vacuum. Few words of very significant meaning occur, and one may surmise that this is due to successful inhibition. A trivial associate, accompanied by a delayed reaction and signs of confusion and embarrassment may indicate that a more significant word was intended, but that it was repressed. Sometimes the introspection reveals that the reaction word given was not the one intended. Such was *fall-down,* when *down* was acknowledged as not the word meant—the real word, however, did not emerge fully into consciousness—and as trivial, and inadequate to the meaning, which was moral, and was admitted to open a complex. Clang associations, such as *fall-call* may be significant in like manner. An incomplete response may indicate a complex, as for instance *seed-peh,* when *pod* was the word intended, and represented by its initial sound, yet pod was not related to the thought tendencies present, which took seed as symbolic. This inappropriateness may have caused the stopping of the pronunciation of the word. A reaction word that merely adds a suffix to the stimulus word may be a substitute for a significant word, as, for example, *ridiculement.* This, in addition, is a nonsense word. Another striking one is *bride-bridle.* An anagram, or near-anagram may even be used, as *bride—bird,* the subject saying that the letters of the stimulus word rearranged themselves into *bird* and she was unable to think of anything else. The tendency to add such suffixes as *ly* and *ing* increases with fatigue, and unless we accept Lipmann's theory, cited on page 130, we cannot regard these as significant when fatigue is present.

12. Last of complex signs to be mentioned as occurring in the course of this experiment is the post-critical response. This is the case when a word insignificant in itself has an inhibited or delayed reaction because of the effect persisting from the word just preceding it. Even when the words were given fully two minutes apart, as they usually were in this experiment, allowing time for the introspection and for taking notes, this phenomenon is occasionally observable. In the majority of observers, for example, the word *new* called forth a delayed response, or perhaps a failure, yet the introspection revealed nothing significant. When it was noted that the word *new* followed *sin,* which nearly always called forth an unusual reaction, and when it was taken into consideration that the train of thought in the after-period might be even more emotional than at the first apprehension of *sin,* an explanation of the unusual reaction to *new* was found.

While it is to be expected that complexes will be found to be individual affairs, depending for their character upon the history and personality of the individual, yet it is well known that some complexes of feeling are universal.

The word *complex* is not here used in its most limited sense, as it is by Freud and Jung, to denote a group of emotionally toned ideas completely repressed into the unconscious, and having a sinister effect upon the mental life of the individual, but rather in the broader meaning given it by Hart in his "Psychology of Insanity" (9). Hart says "A complex is a system of connected ideas with a strong emotional tone, and a tendency to produce actions of a certain definite character" (p. 51). It has been questioned whether this view of complex does not make it really nothing more than what is known as "interest." It is taken by the writer to be something more than, and different from, mere interest, in that it bears a stronger emotional tone, in that it more definitely determines action, and, chiefly, in that it is a system of ideas and an emotion which the individual would conceal from others, although it is not necessarily concealed from himself. It is very likely to be below the level of conscious thought, but the subject may, on the other hand, be fully aware of it, or aware of it in varying degrees. Such groups of ideas tend naturally to gather round the great facts of life, such as birth, marriage, and death. *Complexes,* in the sense just given appear in connection with words relating to these great facts in every observer tested by this experiment. These complexes I have styled "Common Complexes." If the number of persons tested had been large enough to be representative they might have been termed "Universal Complexes." A list of common complexes is given below, with the complex signs shown by each observer with four of the clearest cases. Of course no single word calls out equally strong indications in every observer. On the other hand there are complexes that seem common to all but are not aroused by the same words in all.

Examples of common complexes:

1. Marriage.
   Observer a. Inability to read, delayed reaction, inappropriate image.
   Observer b. Perseverative idea of *two* people, solemn emotion, very short time.
   Observer c. Difficulty in reading, clang association, false reproduction.
   Observer d. Hesitation, time under-estimated, word *tie* automatically.

Observer e. Confusion, a rush of emotional tendencies. Association word *birth*, slightly inhibited. Time long.

Observer f. Embarrassed laugh, hesitation in speaking, no association voiced. "Don't know what I thought of." Clang association when series was repeated.

Observer g. Embarrassed laugh, no association, mechanical motor reaction, time short.

Observer h. Time long. Felt anxious to get a word quickly, "to get it over." *Divorce* came with a distinct sense of relief, as if this were a way out of the trap. (This from the man hater.)

2. Dream.

Observer a. No word, impossibly short time. Thought "Other people dream but I don't." Unconscious of error in reaction.

Observer b. A long blank. Time 4.958. Finally a connection with fiancée, with unusual features in the association. Thought of pressing the key and saying he could think of nothing. The inhibition of association was nearly successful.

Observer c. Prolonged reaction, no definite association, a confusion of thoughts, finally settling to recollection of one significant dream.

Observer d. Slightly prolonged. Word *ghost*. A visual image, which is very rare with this observer. The reaction seemed utterly irrelevant, according to the observer.

Observer e. No reaction word, sense of familiarity. A beginning of a word but it was inhibited.

Observer f. No association, no motor reaction, but a vague idea of dreams in general.

Observer g. Hesitation, blankness, Calpurnia's dream thought of, but no word or image.

Observer h. Premature reaction, without any association. The feeling that this was something familiar and intimate and that therefore the key must be pressed at once.

3. Sin.

Observer a. No associate, a vague reference to "Vision of Sin" and a tendency to say *Longfellow*, though he knew it was *Tennyson*.

Observer b. Spelt word, s-i-n. Distinctly unpleasant feeling, and a groping to get hold of an abstraction. In after period it was thought of as "something you ought not to do." Post-critical reaction.

Observer c. Blank, then rhyme, post-critical rhyme again, and an irrelevant visual image.

Observer d. Redundant response, two words given, both perseverative, both irrelevant. Prolonged time.

Observer e. *Death* as associate, automatic, with no consciousness of connection. After reaction came quotation "Wages of Sin is Death." Post-critical reaction to succeeding word.

Observer f. Clang association, post-critical reaction, one surprising to the observer.

Observer g. The Welt-Schmerz complex indicator, which will be treated under "Individual complexes."

Observer h. Complete inhibition, no reaction. The observer could not understand the inhibition, as it was a familiar idea, "I've been to Sunday School. . . . Everything you do, almost, is a sin."

4. Death.

Observer a. Prolonged reaction time, blankness, a gradual dawn of a visual image connected with brother's death. Non-conscious emotion.

Observer b. Word *youth*. vis. im. of a young girl dead. A feeling that death was hovering all around, and that *youth* made it sad. Long reaction time.

Observer c. Laughter, reaction word *cold*. Feeling of aversion. Faint visual image of tall figure with scythe.

Observer d. Clang association, *dearth*. No feeling. Prolonged reaction time.

Observer e. Reaction word *head*, and sense of its inappropriateness. "All set for a quick reaction, but it was inhibited."

Observer f. Automatic reaction, time .222! A long blank, then vis. im. of a grave yard, and strong emotion. "A shaky feeling,"—"it cast a damper over my whole state of feeling. I don't like to think about it." In repeated series a false reproduction, a solemn, awed feeling, quite strong.

Observer h. Word not understood, so no reaction. This was one of a dozen words given orally. There had been no difficulty in understanding any words, except one other that was also suspected of opening a complex. This observer began to suffer from an hysterical neurosis soon after her father's death. She had nursed her father for eight months.

Other words that are accompanied by one or more complex signs in all or most observers are: coffin, choice, bride, ridicule, friendship, child, quarrel, contempt, expense, size, friend, mate, separation, abuse, delight, prison, pity.

Some words that are in every case met by prolonged reaction time, thought to be due to other causes than *complex* are: green, blockade, custom, water, coat, new, kitchen, suspense, dwarf, pamphlet, contentment, masquerade.

It is not possible to say that none of the second list refers to complexes. It is only said that the evidence is insufficient. Likewise, it is not positive that all in the first list are complex indicators, but the evidence points that way. At the end of this paper will be found a graded list of words, giving the number of observers that reacted significantly to each.

Words are not the most reliable tests, when treated in the manner suggested above, though this is the way most investigators who are not, like Jung, seeking to make diagnoses, treat them. The same word may open different complexes in different persons. On the other hand there are complexes that are common to all, or to nearly all, though they are not reached by exactly the same word in each case. Besides the fundamental complexes already noted, there is the *wealth* complex, common to four of the eight, shown by some or all the words relating to money. The *travel* complex shows some traces in all, strongly in some, when almost every word relating to travel or movement is responded to by the perseverative word *go*, or by a pleasurable emotion. The *ambition* complex, signalized by such words as victory, goal, captain, etc., shows traces in most observers. Words pertaining to out-of-doors, and particularly such words as garden, seed, plant, bring out some strong traces in every observer though it is not the same words that are active in every case. It is quite possible that totally different complexes are touched by these same words. The motherhood complex is evident in three out of five women, and the fatherhood complex in two out of three men. The "*love*" complex, which is not quite the same as the marriage complex—or is not necessarily the same, as the love complex usually arises in connection with a definite 'affair,' whereas the marriage complex exists, either repressed or conscious, in all, without any external object or encouragement,—is clearly evidenced in all the men, but only equivocally or not at all, in the women. This is altogether in accordance with the laws of society, and, we suppose, with the laws of biological evolution. As an example of how results are grouped to obtain clear evidence of a complex one of the love complexes will be given here, because it illustrates the method very clearly, and also because it is quite "harmless," even to a Freudian.

The first word that brought a significant reaction relating to this complex was the word *dream*, already cited in another connection. A very long reaction time, 4.958, an inhibition that threatened to retain the mastery, and difficulty in relating the experience were complex signs. The introspection showed that the word recalled a telephone conversation with a young lady, and afterwards, through later associations the experimenter learned that this conversation was connected with his fiancée. Then came *snake*, with a long reaction time. The image of a *young lady* who is afraid of snakes was all that occurred. In the after period came full recollection of a scene with this young lady, wherein a snake was concerned. Then came *marriage*, which has already been given. There was inability to read the word at first, *as the letters seemed one above the other*, and jumbled together. While we have distinguished between a love and a marriage complex it seems fair to include this word here, as the love complex was so very much in evidence that it would surely have absorbed the other. Then *dear*, association word *deer*, with a laugh, and "knew it wasn't appropriate." "*Knew at once that dear could bring no associate.*" Time very short (.678). A little later the word *friend* brought the following reaction. First, blank (conscious of inhibition), then visual image of a young lady. Consciously trying to find a *person* as an associate. Knew all the time that the intention was toward a person but had to wait for the image to come, time, 1.777. Some fifteen minutes later, on the same day, the climax came with *kiss*. There was a premature reaction—(protective?) but no association given at first. Then the observer began to tell slowly, with evident difficulty, what the word brought to mind, interrupting himself to say, "You ought not to put up such a word." It brought up an experience with the "Young lady." He admitted that this association came at once, that he tried his best to think of something else, but could not, so gave up and confessed. After a few indifferent words came *bride* and this at once aroused the image of the young lady, and all the emotions of the former experience. He "tried to keep it off, saw it was coming, couldn't stop it." The time was longer than the time for *Kiss*. When these words occurred in the repetition series there was no reaction, no distinct recollection of how he had reacted before, but a recollection that "he had had trouble with the words." A few minutes later came *lady* and this aroused the same association, but much more quickly, with no struggle, no resistance, little emotion, and the image was fainter also. The wave was receding. When the word *choice* came, after seven indifferent words, there was complete inhibition for a time, consciousness of this inhibition, and then it resolved itself into making a choice of music—looking over sheet music. The time was one of the longest given by this observer, 2.956. Other words that may have related to this complex but have not very clear complex signs are woman, mate, month, misread as *moth*, butterfly, blush.

The method employed here is obvious, as the complex was obvious. All that was necessary in order to establish this complex was to note what words brought the same associate, although there was seldom an associated *word*, as this observer was of the visual type, and to examine the reactions and introspections for complex signs, which are sufficiently marked. Few other complexes found are quite so obvious, but the general method is the same for all observ [illegible] with clear images or full-fledged thoughts. St [illegible] hal indications. For the type that reacts with words, automatically, with very few images and little thought content, a different method must be employed. Perservation furnishes a principal road of approach here. The perseverated word may be an arbitrarily chosen word used on every occasion when it is desirable to cover a complex—and this concealment may be unconscious—or it may be a word that expresses the emotion which the complex arouses, and so not truly irrelevant, though it bears no external relation to the stimulus word. An instance of the former sort may be *tree*, used to respond to ten words, about half of them showing some possible connection, the other half irrelevant. When *tree* itself occurred as a stimulus word it was misread as *thee*. Of course it is possible that *tree* itself is really a complex word, but the evidence is doubtful. An instance of the latter sort is the perseveration of *hope* in the same observer. A thread of meaning connects the words to which *hope* was responded, although *hope* itself is not an appropriate response to any of them. This observer always gave logical responses except in cases of perseveration, that is, an illogical response was sure to be repeated. In determining complexes in this case perseverated words were the starting point, but no evidence was accepted unless there was more than one complex sign to each reaction considered. Some of the complexes unearthed in this way were the "travel" complex (probably a secondary complex, but this point cannot be established by the data at hand), the "discontent" complex, if we may call it so, for want of a better word—a complex revealed by the perseveration of *hope* and *full*, suggesting that hopes of better things and of wish-fulfillment were entertained, and the sex complex, indicated by several different groups of words. These will be discussed more fully under the head of the Freudian bearing of the experiment. The interpretation given will doubtless look far-fetched unless details are furnished.

The most interesting complexes, if not the most valuable for scientific purposes, are those that are distinctly individual and that give an insight into the character of the individual. While it is worth while to establish by experimental evidence that there are fundamental and universal ideas and emotions, it is no less important, for practical purposes, to discover the individual constellations. Some individual complexes are here cited as of interest and as presenting the definite results of the experiment.

Observer A: Love complex, exposed by 16 words, 33 complex signs, such as prolonged time, premature reaction or short time, embarrassment, laughter, confusion of thought, unsuccessful effort to inhibit, significant response, blankness of mind, surrogate response, irrelevant response, misreading, inability to read. Also "The Lie" complex, exposed by 2 words, with 6 complex signs, such as prolonged time, blankness of mind, inhibition, unusual association, going back to childhood.

Observer B: "Dancing" complex exposed by 6, and possibly 15 words, and 15 complex signs, prolonged time, emotional excitement, significant associate. Also "Reciprocity," or "Twoness," exposed by 9 words and 16 signs, as prolonged time, perseverative idea of two people, emotion, significant association, post-critical response, frank consciousness of the complex.

Observer C: "Garden" complex, exposed by 8 words and 23 signs, as clang associations, redundant response, prolonged time, mixed emotion. (This is a puzzling one. It might be symbolic.) "Friendship" complex, exposed by 4 words and 12 signs, as prolonged time, clang association, irrelevant response, blankness of mind, evidence of repressed emotion.

Observer D: "Travel" complex exposed by 4 words and 12 signs, as perseveration, heterogeneity, redundant response, premature reaction, blankness of mind, automatic response, frank consciousness of complex. Also a "Hope" complex, exposed by 6 words, and 22 signs, as perseveration, prolonged time, misreading, automatic reaction, heterogeneity, irrelevance, some emotion conscious, more unconscious.

Observer E: "Marriage" complex, combined with "Love," exposed by 15 words and 32 signs, such as prolonged time, blankness of mind, confusion, significant association, failure to associate, feeling of being "in a trap," repressions, emotion, acknowledgment of complex. Also a complex connecting with the word "lake," real meaning unknown to experimenter. This was exposed by 5 words and 11 signs, much the same as the "Marriage" and "Love" complex signs.

Observer F: "Death" complex, with 5 words and 12 signs, among which were premature reaction, intense, painful, emotion, blankness, false reproduction, failure to react, failure to associate. A "Love" complex, exposed by 6 words and 13 signs, such as prolonged time, confusion, embarrassment, laughter, clang association, quotation, significant association, failure to react.

Observer G: "Dress" complex, with 6 words and 13 signs, among which were prolonged time, unusual and significant association, surprise, pleasurable emotion, blankness. Also a marked "*Welt Schmerz*" complex touched by 16 words and shown by 54 signs, chief of which was a recurring visual image of a mass of people, accompanied by prolonged time, failure to associate, great emotion, significant association.

Observer H: The "Motherhood" complex, exposed by 6 words and 12 signs, as prolonged time, inhibitions, conflict of associations, strong emotion, redundant and significant response. And also a "Manhating" complex exposed by 6 words and 22 signs, among which were prolonged time, significant response, irrelevant response, blankness, emotion, sense of hurry, failure to associate. (This observer is abnormal.)

V. The next, and last, question to be taken up is that of the bearing of this experiment on the Freudian doctrine. Is there any evidence presented by these results in support of the "complex theory" of Freud? This theory is, in substance, as follows:

1. The unconscious must be accepted as the general basis of psychic life. Experiences are not forgotten, or lost, they are repressed into the unconscious, where they remain active and dynamic. Everything conscious has its preliminary step in the unconscious. Our views, thoughts, and ideals, are all tinged, nay, formed, by these unconscious activities. These find their way out into the conscious life in indirect and hidden ways. Many otherwise unaccountable actions, associations, fancies, errors of apprehension, etc., are explainable as expressions of the repressed wishes, or "complexes" (4).

2. All effectual unconscious complexes are sexual, in Freud's broad sense of the word sexual. However broadened the complex may be to include affections, ambitions, fears, of every sort, there is a sexual component present. The sex instinct is therefore the fundamental instinct. All desires, wishes, and hopes are *derived from sexual desire* (6).

3. The unconscious thoughts, the complexes, all ambitions, wishes, and fears, etc., have their rise in infantile sexual experiences and thoughts. The memory of these is repressed into the unconscious, but the *complex*, i. e., the group of emotionally toned ideas, expressing an unfulfilled wish, remains active, conditioning the conscious processes. Thus is the child father to the man (6).

It is unnecessary here to go into Freud's applications of these principles, since all that we are concerned with is the question whether or not the association method used by Freud himself in exposing complexes, will, when applied by an impartial experimenter to normal subjects, yield evidence in support of these principles. What is their bearing upon the association experiment? Obviously, from data gathered in this experiment, *something* that is unconscious does play a rôle in determining the association of ideas, unless the association is a chance juxtaposition, and psychology has rejected the latter view. There is evidence also that this unconscious is active and dynamic. The complicated train of ideas sometimes recalled to consciousness afterward shows how intricate was the association that came out as an automatic response, often to the surprise of the subject. Associations are sometimes tinged by an emotional tone which the subject is at loss to account for. Also there are physiological manifestations of emotion, apparent to the experimenter without the aid of special testing instruments, manifestations such as quickened breathing, flushing, dilated pupil, fluttering of the hands,—of which the subject is unconscious. This is analogous to the emotion sometimes called forth by automatic writing. These facts, brought out by experiment, lend support to the theory of the dynamic nature of the unconscious, and of its reality as a potent factor in conscious activity. The given associate reveals a *complex*—a group of emotionally toned ideas—at times, of the existence of which the patient is unaware, and he is convinced of its presence only by cumulative evidence. In this experiment there was little opportunity, and it formed no part of the purpose, to acquaint the observer with his own complexes but there was evidence that the observer was unaware of some of his complexes. This evidence was found in the introspection and especially in that part of the testimony elicited by the questions of the experimenter. The observer was sometimes aware of the emotion aroused without being able to explain it. When his associations were brought together and compared the complex stood out plainly, yet the observer sincerely denied its existence. Complexes, in the broad sense in which we have taken the term, are not necessarily painful and not necessarily repressed. An example of a complex that was both painful and repressed was the *death* complex of the neurotic subject. In a more normal subject an example is found in the horror connected with *prison* and in the *friendship* complex of another, who always associated the word *penmanship* (it was tried three times) and who could get no thought of the meaning of the word *friendship*, nor any definite thought except a reference to a friend who was absent. There was no conscious affective tone to this experience, but there was delayed reaction and an unusual absence of thought content. The complex that is fully conscious, and pleasant, is instanced by the *dancing* or *motion complex* of one observer. This sort too may present indirect evidence for Freud's first thesis, but it is to the other sort that we must look for direct support. The complex that is so far repressed that the subject is seldom or never aware of its existence is best explained by the theory that Freud advances.

The second thesis, that all the unconscious complexes have a sexual connotation, receives only a partial and inconclusive support. We can say that there are sexual complexes exposed in every observer—the marriage complex is sexual. We can say that some complexes the main objective of which does not seem to be sexual have also a sexual factor. But this does not amount to saying that *all* complexes are sexual, and all important motives to behavior are therefore sexual, even in the broad sense. This experiment could not be sufficiently wide in scope to establish this point, but if such data as could be obtained were in its favor the fact would be worth noting. A complex traced through the perseveration of *tree* as a response, when tree seemed to express a state of mixed but predominantly pleasurable feeling, in which love of natural beauty was involved, was found to have a sexual component when the seemingly trivial association *fruit-tree* came up. The observer here acknowledged the presence of the sexual complex, otherwise the experimenter would not have been able to detect it. She could have known only that there was a complex of some sort. An observer less frank or less practised in introspection would not have given the information.

The *hope* complex of one observer seemed to be a complex of discontent with the present state of things in general, and the perseverated response voiced the unspoken wish. If one were a thorough-going Freudian one would say that the words to which hope was responded are evidence of a sexual, or Freudian, complex. They are friendship, master, pity, wealth, sin, victory. *Sin* brought a second associate also, in *pain*. But as we cannot proceed on the Freudian assumption we cannot regard these words as giving evidence. The words to which *me* was given as an associate are more suggestive. They are tree (misread as *thee*), dear, kiss, abuse, mate. Indeed, all the complexes shown by this particular observer *could* have a sexual connotation (including the marked travel complex), but there are only vague and inconclusive indications in the associations themselves. It would be necessary to assume the Freudian symbolisms and principles of interpretation in order to see in them incontrovertible proofs of sexual complex. The leaning of the experimenter toward sexual interpretations in this case comes from observations of behavior and of trends of thought, things noted in relationships remote from the experiment, from dreams told by the observer, and other such considerations that have no right to enter into the interpretation of the experimental data. And even if this observer were found to have only sexual complexes (again in a broad sense) it would not prove the theory. There is no doubt that *some* of the subjects examined by the Freudian school fully justify the theory but it cannot be made to apply universally from these few cases. In this experiment, as stated before, all observers give evidence of the presence of at least one sexual complex, in some form, but there is no evidence that the sexual complex is the dominant one, or that every complex has, hidden away in it somewhere, a sexual factor. In the table of complexes given, which did not represent *all* the complexes found, a number of sexual complexes are named. All are susceptible of sexual interpretation if once we admit Freud's principle. But that is just what we cannot do until we have unmistakeable evidence, and we fear that we are not likely to get it by this method.

To the third thesis of Freud's theory the experiment gives no answer. Observers were asked to call attention to any association that they recognized as persisting from childhood. This was seldom done. An infantile root of the association was occasionally uncovered by the questioning of the experimenter but such cases are not in sufficient numbers to be significant. As was shown in the comparison of the normal and the abnormal types, references to childhood experiences are more frequent in the latter than in the former, and the infantile memories seem to play a not unimportant rôle in the complexes of the latter. But the point especially to be decided is that infantile experience plays an extremely important part in the complexes, or constellations, of normal, healthy, men and women. It is a point that an experiment conducted in the manner that this one was can never, so it appears to the writer, determine. Freud's method of uncontrolled " Free association " may lead the subject back to early experiences, helped out by skillful questioning and suggestion. But the word-association used here cannot possibly reach to the deep substratum of the unconscious where these infantile elements lie buried when life is normal and healthy. It is only in cases of mental mal-development that they persist near enough to the surface to be reached easily.

The association experiment, by the word-association method, can hardly be a valid test for the Freudian theories in their entirety. There are many points on which it is silent. The symbolic interpretations of Freud, veiling sexual meanings, may or may not be legitimate. But as these symbolic meanings do not penetrate to the consciousness of the subject without outside suggestion in any case, we cannot expect to find any conclusive evidence for them. Words that might be symbolic—there are many such in the list—are sometimes responded to in a way that suggests the presence of a complex, but without an illegitimate amount of guidance from the experimenter they cannot be made to take on a symbolic significance. The association experiment can probe the unconscious to a certain extent, but it cannot probe to every possible depth. We are not justified in saying that there is nothing more than what has been revealed by experiments similar to this one, neither are we justified in assuming as the truth anything that has not been proved experimentally. The experiment here treated leaves much of the Freudian question open, or it leaves it, to speak more modestly, just where it found it.

VI. This experiment has, when results are brought together, confirmed some facts, brought out by other investigators and has brought up some new data on certain minor points. It has corroborated the statement already accepted by most psychologists, that by the association method mental character may be studied. Many different factors of the reaction, both quantitative and qualitative, enter into the character-indications. It is found possible to establish definite reaction types, with correlation of certain features of the reaction.

In the study of reaction consciousness certain definite features in the reaction come to stand out as 'complex signs.' They are quantitative, qualitative, and physiological. The presence of two or more such signs in any reaction is likely to indicate a complex. As it is seldom that a complex is acknowledged by the observer these signs assume great importance in the exposure of the complex. Certain complexes are in this manner indicated as of common and, it might be inferred, of universal occurrence. These may have innumerable individual variations but there is a fundamental instinct at the base of them. Certain subjects of perennial interest arouse interest and feeling when introduced into the association experiment. They are not *shocking* subjects—the introduction of such would be a gross abuse of the experiment as a means of exposing complexes—but are words in common use and words not evoking extraordinary behavior in every day life. The detached presentment of each word in the association experiment enhances the value of the word, so that it represents a situation to the subject, and he responds accordingly. A part of his behavior to the situation is unconscious, and it is this unconscious part that is most significant. Unconscious mental processes and partly conscious mental processes are much in evidence and the method furnishes a reliable means of "tapping the subconscious" But evidence for the Freudian theory of the unconscious, beyond the first general thesis, is lacking.

If this thesis has anything new to offer it is a new point of view rather than new facts. Hitherto the work upon the complex has been prosecuted from the diagnostic point of view, with the purpose of seeking the individual complex, while the common complexes were disregarded, being taken for granted. In this experiment the principal interest has been in finding out just how these common complexes are manifested in different observers, and how far the association method is of service in uncovering them. A result of the experiment that at least deserves mention, then, is the actual demonstration of the occurrence of certain common complexes in all observers used in the experiment—complexes that manifested themselves in various ways but had certain unmistakeable common characteristics, nevertheless, that could not be concealed by the differences in type.

Of many minor products of the experiment it is impossible to speak, nor has there been opportunity to work out fully all the points suggested by the data. Such points offer numerous incentives to further study, which is their main contribution. The phenomenon of conflicting tendencies in association offers such an incentive. The trend of thought that holds the foreground is very likely to fail of expression, while the association that emerges, either as a word or image, is the result of a trend that was not realized as present until after its culmination. Conflicting tendencies often lead to an inhibition of reaction when no complex is present. The so-called clang associations and rhyming associations are interesting also, as studies. They seem often to indicate a state of resistance to the whole situation, or a *lazy* way of avoiding the effort to associate ideas. They occur oftenest when the observer is bored or fatigued, although they also are sometimes used to conceal complexes. Not unrelated to this phenomenon is perseveration, which, in the results treated here, usually indicated *not* poverty of ideas, but affective mental states. An egocentric attitude usually underlies the perseverative tendency.

A point that there has not been time to work out is that of the general curve in the reaction time of each observer. It has been noted that the time seems to vary most, going to extremes, just after a critical reaction. A question that one would like to see conclusively settled is that of the exact relation of the reaction time to the complex. By the earliest investigators it was thought that a complex always caused a lengthening of the time, but data has since been brought forward to discountenance this view, and, in the opinion of the writer, finally to disprove its correctness.

More detailed study of the data at hand would doubtless bring up more questions, and would perhaps answer a few of them. In conclusion it may be said that this association experiment has proved very fruitful as a means of studying individual psychology, and that it appears to indicate one of the best methods available for the study of a large group of very significant mental processes.

List of words used in experiment:

head; water; green; dream; plant; coat; cigar; kitchen; cold; rose; girl; death; year; wages; window; friendship; master; seed; garden; key; ship; dance; railroad; cave; fight; knife; snake; salt; coffin; mountain; yellow; pity; pain; tree; sky; wealth; sin; new; tunnel; whip; stairway; steamer; wagon; horse; ladder; automobile; blue; voyage; needle; anger; ink; trunk; prison; sickness; lake; village; red; custom; prayer; money; foolish; pamphlet; contempt; finger; expense; bird; fall; book; injustice; black; question; church; organ; captain; victory; bet; frog; separation; hunger; white; child; care; pencil; grief; plum; blockade; suspense; goal; game; marriage; dear; glass; quarrel; fur; size; carrot; paint; part; note; dress; veil; cherry; ace; fruit; age; flower; punishment; box; savage; family; soap; cow; friend; luck; lie; deportment; path; sister; fear; stork; falsehood; diamond; beauty; ring; indifference; costume; lady; knight; anxiety; door; kiss; purity; choice; hay; bride; contentment; ridicule; sleep; month; delight; nest; milk; meat; doll; dwarf; masquerade; woman; abuse; mouth; mate.

Graded word list, showing which words were effectual as complex indicators. The first eight words were significantly reacted to by eight observers, the next eight by seven and so on down:

8: marriage; dream; sin; death; coffin; choice; bride; ridicule.
7: friendship; child; blockade; quarrel; seed; contempt; expense; size.
6: water; coat; girl; flower; anxiety; abuse.
5: delight; prison; pity; suspense; dwarf; kiss; horse; dance; song; game.
4: year; wealth; anger; finger; separation; friend; tunnel; whip; bet; victory; mate; pride; punishment; prayer; diamond; ship; door; dear; ladder.
3: mountain; ring; sleep; stairway; captain; luck; costume; beauty; master; veil; sky; injustice; bright; dwarf; cave.
2: plant; book; ace; box; next; bird; goal; free; cold; fear; wages; knight; family; automobile; purity; money; fall; question; care; lie; trunk; pain; knife; fear; falsehood; ring.
1: month; lady; ship; cigar; master; key; village; lake; sickness; railroad; bet; plum; glass; fur; fruit; age.

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BOOK NOTES

Bodily changes in pain, hunger, fear and rage; an account of recent researches into the function of emotional excitement. By WALTER B. CANNON. New York, Appleton, 1915. 311 p.

In this long desired volume the author sums up the results of his own experiments and gives us a general presentation of the subject. In successive chapters he deals with the effect of the emotions on digestion, the general organization of the visceral nerves concerned in emotions, methods of demonstrating adrenal secretion and its nervous control, adrenal secretion in strong emotions and pain, the increase of blood sugar in pain and great emotion, improved contraction of fatigued muscle after splanchnic stimulation of the adrenal gland, the effects on contraction of fatigued muscle of varying the arterial blood pressure, the specific rôle of adrenin in counteracting the effects of fatigue, the hastening of the coagulation of blood by adrenin, the hastening of coagulation of blood in pain and great emotion, the utility of the bodily changes in pain and great emotion, the energizing influence of emotional excitement, the nature of hunger, the interrelations of emotions, and alternative satisfactions for the fighting emotions.

An introduction to neurology. By C. JUDSON HERRICK. Philadelphia, W. B. Saunders Co., 1915. 355 p.

This is evidently a very welcome and much needed book. It is intended to be and is an admirable "Open Sesame" for the beginner and an excellent summary of what is known to date. The book is illustrated by 137 diagrams, contains an excellent index and glossary and in its successive chapters treats the following topics: the nervous functions, the neuron, reflex circuits, receptors and effectors, general physiology of the nervous system, general anatomy and subdivision of the nervous system, spinal cord and its nerves, the medulla oblongata and cerebellum, cerebrum, general somatic systems of conduction paths, vestibular apparatus and cerebellum, auditory apparatus, visual apparatus, olfactory apparatus, sympathetic nervous system, visceral and gustatory apparatus, pain and pleasure, structure of the cerebral cortex, functions of the cerebral cortex, evolution and significance of the cerebral cortex.

The problem of knowledge. By DOUGLAS CLYDE MACINTOSH. New York, Macmillan, 1915. 503 p.

This impressive volume falls into two parts. The first and longest treats of the problem of immediate knowledge, and under this falls that of acquaintance (or epistemology proper), where the author gives a critique of dualism, idealism, rationalism and finally a constructive statement, which is critical monism. Then follow problems of the ways and means of knowing, or the morphology of knowledge and genetic logic, and the genesis of the apriori. Part second discusses the problem of mediate knowledge, first that of truth or logical theory, criticising intellectualism and anti-intellectualism, and defending critical monism in logical theory, and second, the problem of proof (or methodology) which deals with scientific method. The reader will thank the author for an analytical table of contents that precedes the volume itself.

The natural history of the state. By HENRY JONES FORD. Princeton, Princeton University Press, 1915. 188 p.

Princeton University has for decades had an intense consciousness of Darwinism. This was most acute, perhaps, in the time of President McCosh, and seems to work in an ambivalent way, so that those who now believe in evolution are a little prone to magnify its influence and to stretch analogies to conform to it. This seems to us the case in this book. Sociology and economics have passed beyond what the author calls the Huxleyan position, and the genesis of language is far too shaky a foundation to build anything on. Darwin and Romanes are antiquated authorities now on animal psychology, and the author's summaries, biological, psychological, linguistic, and anthropological, seem to the writer of this note distinctly behind the times.

School hygiene. By LEO BURGERSTEIN. Translated by Beatrice L. Stevenson and Anna L. Von Der Osten. New York, Frederick A. Stokes Co. (c. 1915). 188 p.

The author of this manual is one of the chief leaders in school hygiene and it is a godsend to all interested in the subject to have his views presented in this brief, concise form, with copious illustrations, so that it is no wonder that although the first edition appeared in 1912, we have before us already a third edition. Under The Schoolhouse are treated General Planning and Erection, Lighting, Ventilation and Heating, Classroom and Equipment, Grounds, etc.; under Hygiene of Instruction, Number of Pupils and Curriculum, Hygiene of Different Topics, Homework, Examinations, Boarding Schools. Under Instruction in Hygiene we have School Diseases and Medical Inspection, and Hygiene of the Teaching Profession.

Out of work; a study of unemployment. By FRANCES A. KELLOR. New York, G. P. Putnam's Sons, 1915. 569 p.

This work was prompted by the extraordinary degree of unemployment in the winter of 1914-15. The chapters discuss unemployment among women, children, the labor market, relations of immigration, how America markets its labor, labor agencies, domestic service and the intelligence office, marketing skilled labor, philanthropic and civic experiments, public agencies, government regulations, relief of unemployment, insurance against it, program for America. Besides this the work contains useful appendices on national relief, public work, municipal plans, church programs, insurance, etc.

Character and temperament. By JOSEPH JASTROW. New York, Appleton, 1915. 596 p.

This appears to be the first volume of a series by the author entitled "The Conduct of Mind Series," designed to provide readily intelligible surveys of selected aspects of the study of mind and of its applications. The nine chapters of this work are as follows: The Scientific Approach, The Sensibilities, The Emotions and Conduct, The Higher Stages of Psychic Control, Temperament and Individual Differences, Abnormal Tendencies of Mind, The Psychology of Group-Traits, Character and the Environment, and The Qualities of Men.

The function of social taboo in education. By IVA L. PETERS. 84 p.

This pamphlet is apparently a thesis, and perhaps even is incomplete, but as it stands it is altogether the most interesting study of girls in the teens known to the writer. The author interprets taboo in a very broad sense and has collected many compositions, compiled many data from questionnaires, and does present a most vivid picture of what girls cannot do and the restrictions, prohibitions and even the positive injunctions that hedge in their lives at home, in school, and even in class. Happily, however, taboo is now passing.

The "conscious cross-section"; a realistic psychology. By ROBERT CHENAULT GIVLER. Seattle, Dept. of Printing, University of Washington, 1915. 412 p.

This work consists of five chapters, as follows: terminology, psychological analysis, the sensitive and perceptive organs, the emotional complex, matters and minds. "Either logic or flapdoodle. This is the thesis defended in this book with regard to the analysis of mind." The two ever-recurring items are the special form of analysis used, and the continual reference to deep-seated errors in popular psychology. The author is chiefly indebted to the "Concept of Consciousness" by Professor Holt of Harvard University.

Backward children. By ARTHUR HOLMES. Indianapolis, Bobbs-Merrill (c. 1915). 247 p.

This is an inductive study of backward children presenting in a series of concrete illustrations the studies of cases to exemplify principles and methods of diagnosis, treating and training. It describes measuring rods for children, varieties of backward children, typical retardation due to physical defects, minds in straight jackets, bad and backward, retardation due to environment, the backward child in the home, clinical diagnosis, the teacher's diagnosis, the teacher and equipment for a special class.

What may I hope? An inquiry into the sources and reasonableness of the hopes of humanity especially the social and religious. By GEORGE TRUMBULL LADD. New York, Longmans, Green, 1915. 310 p.

This is the fourth in a series of volumes of which the first three have previously appeared, "What Can I Know?" "What Ought I to Do?" and "What Should I Believe?" The chief themes are the nature and sources of hope, rights and limitations of hoping, the assurance of hope, the practical uses of hoping, concerning hopes, scientific, political and social, the hope of moral perfection, the hope of immortality, and the hope of a divine kingdom.

Recognition: a logical and experimental study. By ROBERTS BISHOP OWEN. Psychological Monographs, Vol. XX, No. 2, October, 1915. 154 p.

The author gives first an historical résumé of theories, then a discussion of logical aspects of the problem and consequent assumptions relative to experimental technique. Then come the experiments, as to the recognized content being perceptually present, the time of judgment, voluntary recall and the prompting method, opportunity for intellectual analysis limited, the effect of congruity with context, memory and imagination.

Lehrbuch der experimentellen Psychologie für höhere Schulen und zum Selbstunterricht. Von Joseph Fröbes. Erster Band, Erste Abteilung. Freiburg i. B., Herdersche Verlagshandlung, 1915. 198 p.

After discussing the goal and the way of empirical psychology, the first section treats sensation in general; the second, the single sensations and other elements, sight, hearing, smell, taste, skin sensations, kinaesthetic, aesthetic and organic sensations, and the simple sense feelings.

The school and the immigrant. Edited by Albert Shiels. New York, Department of Education, Division of Reference and Research, Publication No. 11, 1915. 96 p.

This is a series of practical articles by six writers, treating the problem as a whole, methods of teaching English to foreigners, instruction in civics, the occupations of foreign workers, development of social and recreational life, and naturalization.

Nietzsche's moral aim. By William Mackintire Salter. (Reprinted from the International Journal of Ethics, January and April, 1915, pp. 226-403.)

These two articles constitute an admirable epitome of and introduction to Nietzsche, with adequate notes and bibliography. Of the many interpretations of Nietzsche this certainly is one of the most sagacious.

The thinking universe; reason as applied to the manifestations of the infinite. By Edmund E. Sheppard. Los Angeles, Calif., The Authors' Co. (c. 1915). 347 p.

"The aim of this work is to make comprehensible the Grandeur of the Immobile, Intangible Infinite, reposing Majestically in its Eternal, Unchanging Stillness, in Everything, as Everything, propelling Everything."

Subject and object. By Johnston Estep Walter. West Newton, Pa., Johnston & Penny, 1915. 184 p.

This work consists of four chapters, discussing respectively the subject or soul, subject and object in their relation, the nature and our perception of matter, and truth.

Life and work of Pestalozzi. By J. A. Green. Baltimore, Warwick & York, n. d. 393 p.

The teaching of drawing; its aims and methods. By S. Polak and H. C. Quilter. Second impression. Baltimore, Warwick & York, n. d. 168 p.

The Institution Quarterly. Vol. VI, No. 1. Springfield, Illinois, March 31, 1915. Edited by A. L. Bowen. 236 p.

International clinic week at the New York Polyclinic Medical School and Hospital, during the International Surgical Congress, April, 1914. By Alfred C. Jordan and others. New York. 103 p.

Aristotle on his predecessors; being the first book of his Metaphysics. Translated by A. E. Taylor. Chicago, Open Court Pub. Co., 1910. 159 p.

An enquiry concerning human understanding, and selections from *A treatise on human nature.* By David Hume. Chicago, Open Court Pub. Co., 1912. 267 p.

Internationale Zeitschrift für ärztliche Psychoanalyse. Hrsg. von. Sigm. Freud. II. Jahrgang, 1914, Heft 1, Jan.; Heft 3, Mai; Heft 5, Sept. Leipzig, Hugo Heller & Cie, 1914.

The pragmatic advantage of Freudo-analysis (a criticism). By Knight Dunlap. (Reprinted from the Psychoanalytic Review, Vol. I, No. II, February, 1914, pp. 149-152.)

Occupational neuroses; pathogenesis and examples of treatment. By Tom A. Williams. (Reprinted from the Cleveland Medical Journal, July, Vol. XIII, 1914, p. 447.) 15 p.

L'Année Psychologique. Publiée par Henri Piéron, Directeur du Laboratoire de Psychologie physiologique de la Sorbonne. Vingtième Année. Paris, Masson et Cie, Éditeurs, 1914. 545 p.

Boletin de la Sociedad Española de Biología. Marzo-Abril, 1914. Año IV, Núm. 27. Madrid, Imprenta de Hijos de Nicolás Moya, 1914. 100 p.

Fortschritte der Psychologie und ihrer Anwendungen. Hrsg. von Karl Marbe. II. Band, IV. und V. Hefte. Leipzig, B. Teubner, 1914.

The negro races; a sociological study. Volume 1, the Negritos, the Nigritians, the Fellatahs. By Jerome Dowd. New York, Macmillan Company, 1907. 493 p.

Journal of Philosophy. July 10, 1913-December 10, 1914. Numbers 317-331. Published by the Philosophical Society (Imperial University), Tokyo, Japan. (In Japanese.)

Le respect mutuel. Par Pierre de Coubertin. Paris, Librairie Félix Alcan, n. d. 104 p.

A horizontal-vertical illusion of brightness in foveal vision apparent in astronomical observations of the relative luminosity of twin stars. By Joseph Wanton Hayes. Psychological Monographs, Vol. XX, No. 1, August, 1915. 126 p.

Madison, "The Four Lake City." Recreational Survey. Prepared by a special committee of the Madison Board of Commerce (c. 1915). 103 p.

Treballs de la Societat de Biologia, Any Segón, 1914. Publicats sota la direcció de A. Pi Suñer. Barcelona, Institut d'Estudis Catalans, Palau de la Diputació. 279 p.

THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded by G. STANLEY HALL in 1887

VOL. XXVII APRIL, 1916 No. 2

THE TRIDIMENSIONAL THEORY OF FEELING FROM THE STANDPOINT OF TYPICAL EXPERIENCES[1]

By W. S. FOSTER and K. ROESE

This study is a supplement to the experiments by the method of paired comparisons made by Titchener[2] in 1902 and by Hayes[3] in 1906. The results of these experiments are later summed up as follows:[4]

> "(1) Judgments of pleasantness and unpleasantness are direct, easy, and natural. The qualities themselves appear to the observers to be simple and homogeneous, identical throughout the experiments. Their opposite character is vouched for both by introspection and by the course of the curves.
>
> "(2) Judgments of excitement are less direct, and the term is equivocal. If it is taken as the opposite of depressing melancholy, its curve agrees with that of pleasantness; if it is taken as the opposite of tranquility or soothing calm, its curve agrees with that of unpleasantness: the reverse curves then agree with those of unpleasantness and of pleasantness, respectively. If, in default of special instruction, the observer vacillates between the two meanings of the word, the curve shows a vacillating character,—partly 'pleasant' and partly 'unpleasant;' the period and nature of the affective oscillation are vouched for by introspection. Judgments of depression are, in their turn, distinctly less direct than those of excitement, and are often associatively mediated. There is no evidence of a dimension of excitement-depression, still less of a number of exciting and depressing qualities.

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[1] From the Psychological Laboratory of Cornell University.
[2] E. B. Titchener, *Philos. Stud.*, 20, 1902, 382-406.
[3] S. P. Hayes, *Am. Journ. Psych.*, 17, 1906, 358-393.
[4] E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, 165 ff.

"(3) Judgments of tension are easy; but tension is described, throughout, in kinaesthetic terms. Increasing tension means, uniformly, increasing unpleasantness, and the curves of the two classes of judgment correspond. Relaxation may be taken as the opposite of unpleasant tension, in which case its curve agrees with the curve of pleasantness, or may be identified with depression. Nowhere is there evidence, in this third case, either of a new affective dimension or of specific qualities.

"Naturally, these results are not 'conclusive.' For one thing, the experiments are too few. For another, they were obtained in a single laboratory, and that a laboratory from which criticism of Wundt's doctrine had already proceeded. For a third, the argument upon which the experiments rest is not demonstrably valid. It would, I think, be a very strange thing if three sets of stimuli should affect a number of observers by way of excitement-depression (or tension-relaxation) precisely as they do by way of pleasantness-unpleasantness,—but nobody can prove that such a state of affairs is, on the plural theory, impossible. Were I a champion of affective plurality, I should unhesitatingly urge these objections to the work, and I have no desire to slur them over because I am on the other side."

We believed it worth while, therefore, to add to the number of the experiments; and also to introduce into the conditions of experiment certain variations which might make for results more favorable to the tridimensional theory, provided that the theory is valid, or might at least cast additional light upon the nature of the feeling qualities. We have, for example, obtained two observers who were not only quite unpractised, but were also entirely unversed in theoretical psychology, so that the charge of laboratory bias can hardly be brought against them. Further, we have given all observers concrete daily experiences illustrative of the feeling-dimensions in question, thus trying to avoid the ambiguity of purely verbal orientation, and in its place to set up not only a constant but also a correct standard for all six of the Wundtian quality-groups. Furthermore, we have asked for judgments of pleasantness and unpleasantness only *after* the series in the other dimensions were completed, so that previous practice should give no suggestion towards an identification of pleasantness-unpleasantness with other feelings.

Method. We used the same harmonical and the same series of twenty-four clangs employed in the former experiments (C-c, c^1-c^2, c^3-c^4). The c did not sound especially loudly, as it did in Hayes' experiments. We took the usual precautions in arranging the order within the stimulus-pairs. Hayes' and Titchener's curves, and repetition of our own depression and strain series in reverse orders, show that the order (higher tone first or higher tone second) makes in general little difference to the course of the curves obtained. The series in the other dimensions were therefore given but once, and then in haphazard order, care being taken merely that no tone should be oftener the first than the second of a pair. We gave the stimuli of a pair in the same time relations as did Hayes: 'Ready,' interval 2 seconds, tone 2 seconds, interval 2 seconds, second tone 2 seconds. Five seconds were allowed between successive pairs in all series except the strain series, where the observers declared the observation to be especially fatiguing, and a 10-second interval was at their request adopted. Rests of approximately 2 minutes were given after every 20 pairs, and the observers then jotted down such observations as occurred to them.

Before the experimental series of a given dimension, an hour—and before the work of a given hour, a period of several minutes—was spent on special training, intended to make apparent exactly what was meant by the term designating the quality to be judged. For *depression*, the observers were taken several times from the light and shut for a few moments in a dark room.[5] They were told not to think especially of anything, but to give themselves up to the impression which the darkness should make upon them. This total impression they were told to understand as depression, and they were asked to judge the tones in the light of this experience. We hoped that in this way a depression in Wundt's sense might be secured, and might serve as a constant standard of judgment throughout the experiment. For *excitement* the observers were similarly brought from a dark room into the light. As a preliminary to the *strain* experiments, the observers listened attentively to metronome clicks given at three-second intervals, and were told that the impression of the waiting period was to be understood as strain. For *relaxation* the metronome was again used, and the observers were told that the impression given them by the coming of the click and at once thereafter was the impression in question. For *pleasantness* the orientation was given by the taste of a quarter-teaspoonful of sugar syrup, and for *unpleasantness* by the taste of a quarter-teaspoonful of a 0.1% solution of sulphate of quinine. Subsequent test-experiments, in which two observers were given peppermint and certain perfumes to smell, tickled with a camel's hair brush on the lip, shown saturated colors in the dark, asked to perform mental multiplication, etc., and to tell what feelings were aroused in these cases, gave results in close accordance with Wundt's statements,[6] and indicated in a preliminary way that our illustrative experiences tended to give a correct notion of the nature of the six quality-groups. After the close of the complete experimental series for every quality-group, the observers were given a few repetitions of the regular and of the preliminary exercises, and were asked to characterize the impressions more completely, and especially to say what was common to both the preliminary experiences and the tonal experience, and to state (so far as possible) the basis of their judgment.

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[5] See W. Wundt, *Grundzüge der physiol. Psych.*, ii, 1910, 295 ff.

[6] *Loc. cit.* The sole exceptions were that one observer called blue relaxing as well as depressing and pleasant, and red slightly straining as well as exciting and pleasant.

Observers. The one of us (R) served as experimenter throughout the experiment; the other (F) served as an observer, and is responsible for the formulation of results as expressed in this paper. The observers may be divided, on the basis of psychological knowledge and experience in psychological observation, into three groups. F, the writer, and Dr. J. N. Curtis (C), fellow in psychology, were highly practised observers; Mr. S. S. George (G) and Mr. F. L. Dimmick (Di), graduate students in psychology, had had considerably less practice. Miss E. Alspach (A), a senior, was forced to discontinue work after a single series on account of ill-health. Miss H. Hosmer (H) and Miss H. Kinnear (K) were freshmen with no psychological experience whatever. They had not even read a psychological text. The first four observers completed series in the order depression ↑, depression ↓, strain ↓, strain ↑, and then relaxation, excitement, pleasantness, unpleasantness, haphazard. The last two observers completed depression ↑, depression ↓, strain and pleasantness haphazard, and H at a later time also completed a second strain series.

Results. The curves for all observers are shown in Plate I. Single curves in the cases of D and S represent averages of ↑ and ↓ series. The use of the average is justified by the fact that under our conditions the ↑ and ↓ curves were quite similar; the average difference between corresponding ordinates of the curves for D ↑ and D ↓ was: for F, 1.3; for C, 0.3; for G, 2.7; for Di, 0.8; for H, 3.1; for K, 1.9. The figures for S ↑ and S ↓ were: for F, 2.1; for C, 1.5; for G, 1.5; for Di, 1.4; for H, 3.1. The greatest possible average difference (complete opposition) would evidently have been 11.5, and the least 0. If we express the general difference as a ratio of actual to possible difference, then F's D-curves have a general difference of 0.11; C's, of 0.03; G's, of 0.24; Di's, of 0.07; H's, of 0.27; K's, of 0.17; and the figures for the S-curves are F, 0.19; C, 0.13; G, 0.13; Di, 0.12; H, 0.27.

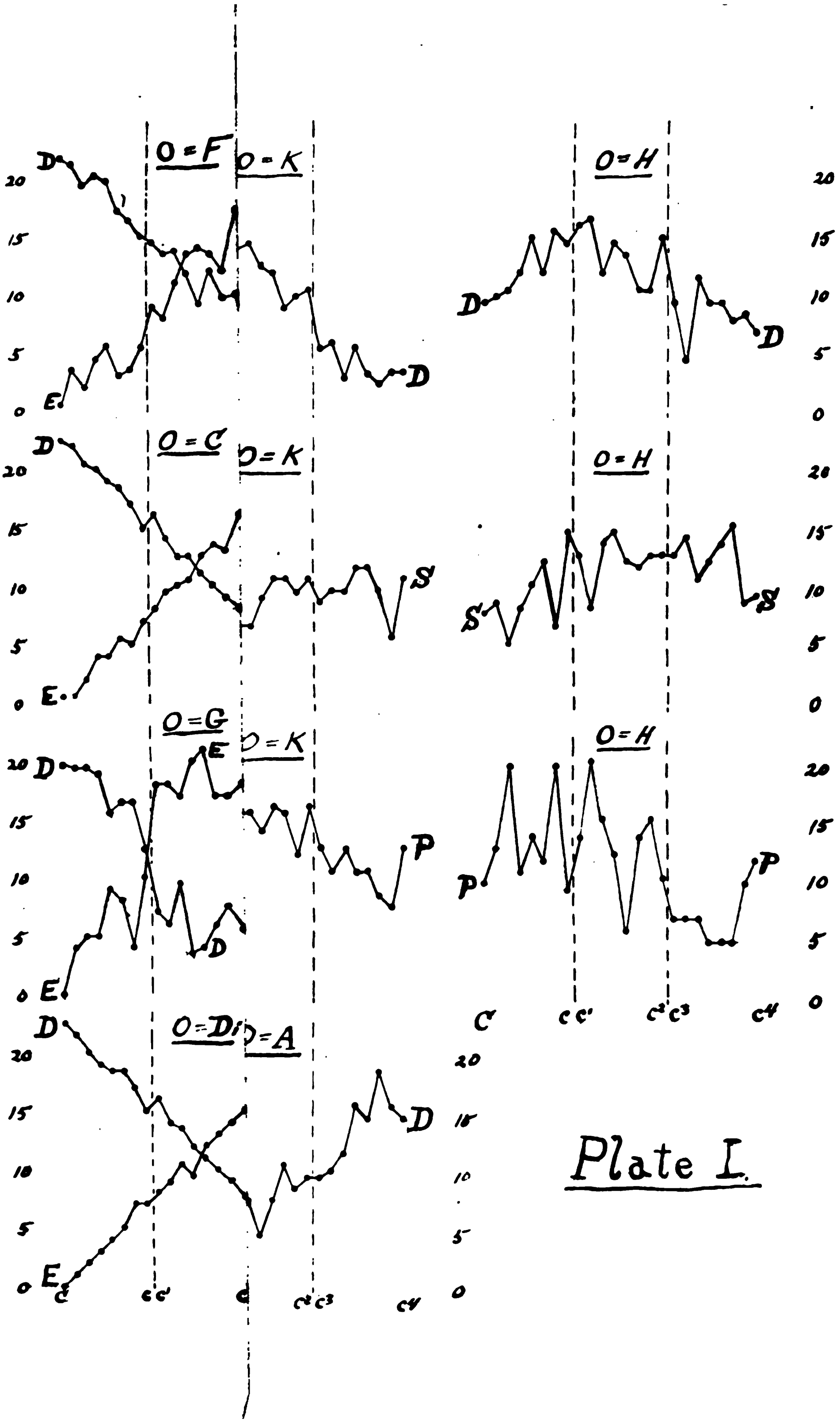

Plate I.

The first fact to be noted, therefore, is the constancy of judgment of all our observers, where repetition of series involving a single quality-group is concerned. This constancy is greater than was obtained in a number of Hayes' series. The general difference of the P ↑ and P ↓ curves of Hayes' observer M, for example, even after considerable previous practice, is 0.96, and other large general differences may be found elsewhere in the work.

Secondly, from a glance at the curves of each pair of qualities, it is evident that under our conditions E and D, S and R, as well as P and U, are nearly exact opposites, a result which also did not appear uniformly in previous work.[7] We believe that for this outcome, as well as for the above mentioned constancy, our repeated orienting exercises are responsible.

Thirdly, we may point out that the curves which we have obtained belong in general to the types previously found. Inspection led us to believe that three typical pairs of curves could be made out. We may call them for convenience the X, the OX, and the XO pairs, exemplified respectively in the D-E pair of F, the S-R pair of G, and the P-U pair of G. For the sake of classifying doubtful cases, we further defined these pairs in terms of the relative amounts of the sum of the ordinates within an octave.

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[7] We may measure the degree of opposition as well as the similarity of our curves in terms of the general difference above referred to. Complete similarity is thus expressed by a general difference of 0 (=0/11½), neither similarity nor opposition by 0.5 (=5¾/11½), and complete opposition by 1.0 (=11½/11½). The expression of general difference, however, becomes more convenient if we transfer to a scale in which +1.0 represents complete similarity, 0 neither similarity nor opposition, and —1.0 complete opposition. In general form, the steps in the method are then as follows: (1) sum up the individual differences of corresponding ordinates, taken without regard to sign; (2) divide this sum by the number of ordinates *times* the amount of the average ordinate; and (3) multiply this quotient by 2, and subtract the resulting product from 1.0. We have not as yet had opportunity to compare this method fully, either empirically or theoretically, with the standard methods of correlation. From the few cases in which we have made comparisons we get the impression that the Spearman method of rank differences and the foot-rule method especially, but probably the Pearson method also, in some cases at least give misleading results when used to measure similarity or opposition, as we here understand the significance of these terms. Even if this impression proves mistaken, our method finds a warrant for use in this particular case by merit of its simplicity. This question, and the question of the applicability of the method to other conditions than our own, we are forced to leave for future consideration. We may note further that we have measured by this method the oppositions and similarities stated by Titchener and Hayes in a number of possibly doubtful cases, and in every case the mathematical result verifies the judgments made by them on the basis of direct inspection.

If the order of these sums from left to right is great-medium-small (g-m-s), or small-medium-great (s-m-g), the curve belongs to the X type; if medium-great-small (m-g-s), or medium-small-great (m-s-g), to the OX type; if great-small-medium (g-s-m), or small-great-medium (s-g-m), to the XO type. The following Table shows the type-distribution of the 104 curves of our own and the previous experiments in terms of these six types:

		D	E	S	R	P	U	Total
X	g-m-s	12	3	0	9	10	0	34
	s-m-g	4	8	10	0	0	10	32
OX	m-g-s	2	2	0	2	4	0	10
	m-s-g	3	3	6	0	0	4	14
XO	g-s-m	4	1	1	0	0	1	7
	s-g-m	1	2	1	0	3	0	7
Total.	....	24	19	18	11	17	15	104

This classification is, of course, to a certain extent arbitrary. By very definition it forces into a typical form, not only all our actual curves, but also all possible curves which we could have obtained. Its first justification (1) is that inspection led us to believe that a great majority of the curves fall into one of six such general forms. Only in a comparatively small number of cases was it necessary actually to compute the sums of the ordinates when determining their order. Still further justification for the classification, however, was sought. (2) After classifying the 104 curves according to type, we computed the average curve for each type, together with the mean variations of the 24 ordinates in each one of the six cases. Although a determination of the maximal variation possible without invalidation of type is undetermined (and, so far as our mathematical knowledge goes, undeterminable), the actual variations were so small that we consider them reliable evidence in favor of the existence of the six types. (3) Furthermore, small as were the mean variations, we were impressed by the fact that their amount was often determined, not so much by the large number of values closely approximating the averages, as by a few values which departed rather widely from it. We determined, therefore, to compute also the *median* deviation of the ordinates in every case; and thus, if these latter turned out to be less in amount than the mean deviations, to assure ourselves by this comparison that there was even greater evidence for the typical curve than the smallness of the mean variations indicated.

The results were astonishingly unanimous in confirming our impression. In the X pair, out of 48 (2x24) cases, the mean variation exceeds the median deviation in 45, and equals it in 2; for the same number of cases in the OX pair, the corresponding figures are 34+7, and for the XO pair, 39+4. In the total of 144, the mean variation exceeds in 118, equals in 12, and falls below in only 14 cases. Hence 0.82 (=118/144) may be taken as a rough representation of the tendency of our curves to cluster inside their mean variation from the typical curve.

The average mean variation of the g-m-s curve is 1.7, its average median deviation 1.2. Corresponding figures for its opposite (s-m-g) are 1.9 and 1.4. Less difference is apparent in the curves of the other pairs, though it still remains definite: m-s-g, 2.2 and 2.0; m-g-s, 2.2 and 1.9; g-s-m, 1.9 and 1.4; s-g-m, 2.4 and 2.1. This comparison, therefore, appears to us to show definitely a tendency of the individual curves to group closely about the average curve, thus justifying further our distinction of types.

Plate II shows the three pairs of average curves (the heavy lines). Above and below every ordinate of a curve its individual median deviations have been laid off, and these points have been connected by dotted lines. The space between the dotted curves thus formed, represents, probably more accurately than any other form of graphic illustration, the median range over which the variation from type takes place. The sum of the average median deviations for the six curves is 10.0. The maximal possible ordinate of our curves is 23.0. Hence $1 - \frac{1}{2} \times 10/23 = 0.78$ may serve to represent the definiteness with which our curves fall into six types.

We turn now to the introspective reports of our observers. As one reads them, their relevance to the corresponding curves (Plate I) becomes for the most part self-evident. We withhold interpretative comment here, remarking merely that many of our quotations are made from the introspections taken after a regular series was completed, because at that time general statements were practically required. It is to be remembered, however, that such report was never given from memory alone, but was checked in every instance by actual experiment.

Depression and Excitement.

F. Darkness is "heavy, big, dull, inevitable." It "presses down on my chest and shoulders," and "seems to take the tonicity out of my muscles," "makes me feel quiet, subdued, passive, heavy," "throws me into a mood of 'don't care' or 'there's no use of doing anything.'" "The experience on the whole is rather pleasant." "Coming into the light gives me the feeling of 'lightness' and 'liveliness' or 'readiness,' the impression of heightened tonicity of the muscles." "My shoulders actually seem to rise and I feel lighter in weight." "This experience is also 'slightly pleasant.'" Judgments of depression and excitement with the tones are very easy. "The first stimulus merely puts me into a state, and the second 'lowers' or 'raises' and 'quiets' or 'arouses' me more or less than the first."

C. "Darkness makes me feel slow, subdued and serious." "It weighs me down like a weight on my shoulders and depresses me." The light made her feel "cheerful, springy, and light in weight, as if floating up instead of being pushed down." "Excited is too strong a word: it (light) is rather cheerful, quick, and to a slight degree pleasant." "The high tones aren't depressing at all." "The low tones sound almost the same way the dark room felt, i. e., they are serious, sober and subduing." "They are heavy and pressed on my chest, and made my breathing slower and deeper." (Afterwards remarks, however, that "all tones, even the low ones, are more cheerful than the darkness," and that "the low tones are pleasant and the darkness neither pleasant nor unpleasant.") Judgment, she maintains, is made on the basis of the change ('drop' or 'rise') of kinaesthesis in the chest.

G. Evidently takes depression and excitement in the sense of unpleasantly depressing and pleasantly exciting. "Darkness is sad, lonely and fearful, and takes control of me and makes me feel powerless." "The low tones are harsh and merciless, and shut close around you like the darkness, and they are sad and lonely." "I dislike them." "The middle tones give one a social feeling, and differ from the darkness in being pleasant and in not giving one a lonely feeling." "The high tones close in on one as the darkness does." "They seem lonely and distressing." "They are not unpleasant, but are like darkness sad and embarrassing."

Di. Found great difficulty in expressing the basis of his judgment in all six dimensions. He was at the time of the experiments also serving as an observer in experiments to determine the limens of pitch and volume of tones, and maintains that he could not entirely free himself from the 'sensory' attitude. "Cannot see that either low tones or darkness make more than a direct sensory impression on me." "They do not arouse what I should call feelings in me." "I cannot find any difference between the feeling before, during and after the tone, except in the direction of attention." "Would not ordinarily say that either the light or tones were in the least exciting." He "cannot see that the tones are like the darkness (or light) at all, except both are more or less big, diffuse, and roomy." This character is what he "has to understand depressing to mean."

K. "Darkness is heavy and solemn and makes me downhearted; is tiresome." "It is not quite boresome, but the *sameness* of it

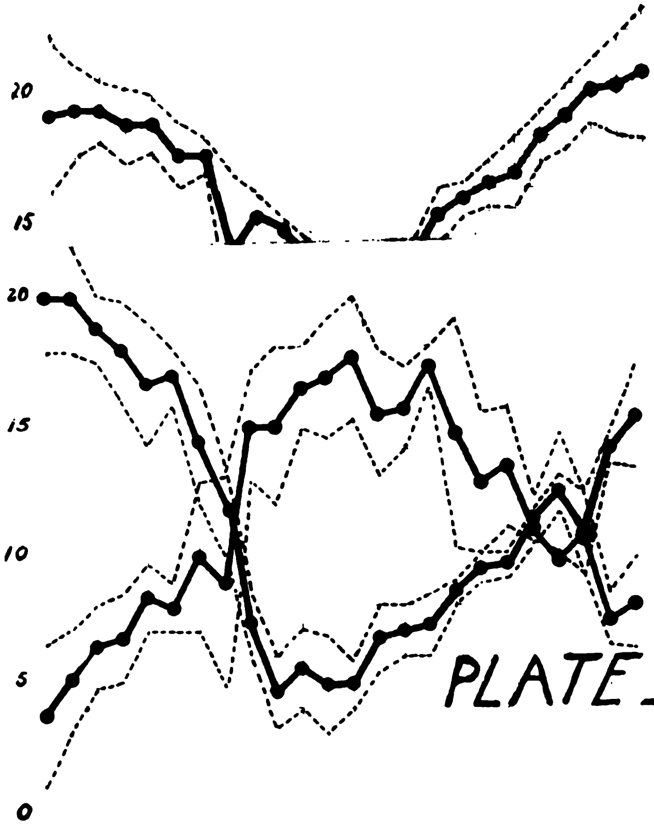

PLATE II

bores me." "The lower tones are mournful and deep and heavy." "They seem to fit in with the mood of the darkness." Medium tones "have a soothing effect and are a relief after hearing low tones." "They are more musical and pleasing." "Low tones have a tiring effect which medium tones do not have." "The high tones make me nervous." "They would seem out of place in the darkness, and generally have no depressing effect at all."

H. "Darkness seemed soft and heavy, and felt as if it belonged particularly to me," "rather inspiring." "It was not depressing particularly; not dreadful, but just serious. . . . . In it I was happy in a quiet way." The low tones "seem far away and not so personal." "They are not at all inspiring and seem to run through and through me." "They seem thick and rough and blunt." "The medium tones seem nearer and more full of meaning." "They are more like the darkness than the lower notes because they are not so grasping." "The high notes are annoying and stick in my mind more unpleasantly."

A. "The judgments are hard to make because none of the tones press in upon me as the darkness did." "I seem to have to make my judgments on the basis of dislike."

Strain and Relaxation.

F. "Strain for me is attentive activity, tightness." "While waiting for a click I feel forced, held attentive." "The higher tones hold me attentive and strained." "This strain is 'thinner' than the strain of waiting, even though it may be more intense." "The strain seems localized primarily in my chest (holding breath or breathing tightly) and about my ears." "Relaxation is passivity, laxness, relief, freedom." "When the click comes, it lets me go, I sink down." "It is different from depression in that I am not *pressed* down, but 'drop' down of myself as far only as I want to go." "It is rather looseness, laxness, especially in the chest, and the fact of 'downness' doesn't matter."

C. "Strain is effort, tightness, tenseness, with irritation or impatience." "In waiting for a click there is a tension all over, especially in the muscles of breathing and around my eyes and ears." "The low tones are not accompanied naturally by strain." "In order to compare low or medium tones I have to institute a course of kinaesthesis, which resolves into the strain in my throat necessary to sing them; I have then to base my judgment either upon throat kinaesthesis or upon which tone I would prefer to have go on if one of them had to." "The strain of the high tones is much like the waiting strain in being irritating." "Aside from breathing, strain seems to be chiefly a matter of ear and throat kinaesthesis, the former in the case of high tones and the latter in the case of the low ones." "I can't get any general attitude except that of these strains and a sort of general irritation." "Relaxation judgments are much easier than strain, though not as easy as depression." "Relaxation is relief, restfulness, and a failure of any activity on my part." "It involves a general relaxation of the muscles which have been strained in expectation or by previous tones."

G. "Low tones are not straining at all; there is no impatience with them. You tend to exhale instead of inhaling or holding your breath, as you do in waiting for a click." "The [low] tones seem to press one down, and waiting and the high tones, to pull one up." "The muscles of themselves act independently of your activity listening to the tone." "The middle tones are pleasant and leave one normal." "I feel strain quickly if at all, but it takes some time for relaxation to come." "I seem to feel strain naturally, but relaxation is a secondary thing I have to tell myself to get. It is hard to keep the determination constant." "The [relaxation] judgment seems to me to be organic sensations and pleasantness."

Di. "Strain is expectancy, attentiveness, with strain sensations especially from respiration and in throat." "The tones simply came to me with strain or without." "The whole thing seemed very objective, the strain seemed to be the strain of the *tones,* rather than strain which I exerted." "Relaxation is simply lack of strain or less strain." "The more relaxing tone is the one which doesn't compel my attention." "There is nothing positive that I should call relaxation about the tones."

K. "The tones had the same effect as while I was waiting for the click. It was an impatient feeling, or as if something was still coming and I couldn't easily wait." "I am tense, and more or less impatient. There was a feeling of relief when it was over." "The middle tones didn't leave such a feeling of relief as the low or high ones did." "The more straining tone was the one which left the greater relief after it had gone."

H. "While waiting I feel tense, alert, nervous and annoyed." "The feeling was rather blank, but certainly straining." "The low tones do not seem to arouse much activity in my mind." "The middle tones produce the same feeling of strain [as the waiting] only less of it." "The high tones seem as if they might be straining, but some of them are too high for my mind to really sense them. They seem too small and indefinite." "I sometimes called the tone more straining if it left the greater feeling of relief."

Pleasantness and Unpleasantness.

F. Finds pleasantness "very hard to judge." "The sugar pleasantness is milder, smoother, calmer, evener, than the pleasantness of the tones, even the best of them." "It is hard to keep the affective set correct, or seems to be. The sweet made me tend to smile, and so do some of the tones more than others. . . . The pleasantness of the middle tones seems to me different from that of the low tones, though I can't say just how." Later: "Tremendously hard to judge between the moderately high and low tones. Both are slightly pleasant but the pleasantness seems different in the two cases: in the first, smaller and more definite; in the second, larger and more diffuse, somehow. In general, the pleasantness of the lower tones seems more like the pleasantness of the sweet." "The middle and lower tones are smooth, sweet and smilingly pleasant, like the sugar." "The pleasantness of the tones and sugar seems more in the tones and the sweet themselves than did depression, excitement or strain; the latter three seemed to be more my reaction." "The pleasantness seems to be more 'in my face,' and the depression, excitement and strain were more 'in my chest.' . . . I do not mean that I could localize absolutely definitely in either case." (In judging unpleasantness) "All tones except the very high ones seem pleasant, I have to translate them out of pleasantness-terms and say: 'This is less pleasant,' sometimes." "I think that even the high tones are not exactly unpleasant like the bitter, but rather unpleasant like sour. The low tones seem a little like bitter and sweet together, perhaps pleasant like chocolate." "Unpleasantness of the tones is more their sourness than their bitterness. I can't make the high tones seem more than a little like bitter. They do tend to make me wrinkle up my nose and half sneer at them, somewhat as I tend to do at bitter and sour."

C. "Pleasantness [of sugar] seems like a warm glow over the upper part of my body." "The very first tone we had [*d*] felt just the same as the sugar." "It is quite hard judging between very high and very low ones. The low ones are diffuse like the pleasantness [of sugar] but they are also heavy and the pleasantness doesn't seem heavy at all. If I were going by the criterion of which I'd rather have go on, I'm sure I'd choose the low in preference to the high." Later: "Think I am set better than I was at first. I don't bother about all the stuff above. I simply don't like the high irritating tones, they seem to have a nasal twang that is unpleasant." "Pleasantness is more like relaxation than any of the other feelings we have had." "Unpleasantness is quite an easy judgment." "The bitter taste is quite unpleasant, so are some of the tones. Not sure whether it is the same unpleasantness. One big part of it seems to be kinaesthesis, only for quinine it is localized about my mouth and for the tones it is my whole body trying to get away." "The low tones are nice and cool."

G. "The sugar was not as pleasant as I thought it would be. What is more, I seemed to forget all about this pleasantness when I began to judge, and depended on my general attitude of pleasantness, rather than on that of the 'sugar pleasantness.'" "The sugar pleasantness was more or less too localized to be general pleasantness, which is more 'exciting' than that of sweet sugar." "The ['exciting'] pleasantness seems more to conform with that of the tones." "Unpleasantness is harder to judge than pleasantness. I sometimes feel at a loss and have to resort to all sorts of criteria other than unpleasantness, as straining, sad, shrinking, and all sorts of things." "None of the tones cause unpleasantness as we ordinarily understand that term." Later: "I am still having trouble with the judgment, as usually both of the tones are pleasant. The judgment comes to be merely the calling to mind of secondary criteria which the tones arouse, and calling these pleasant or unpleasant."

Di. "I am very dissatisfied with these judgments [of pleasantness]." "The pleasantness I am told to judge is not the same as I mean by pleasantness. Have to build up a criterion for the pleasantness of tones which is comparable to that of taste. The judgment seems to be not of pleasantness, but of which tone is sweeter." "The low tones have a heavy, almost sickly sweetness like honey. Big heavy tones sound like thick, rich sweets; I don't know why." Later: "Pretty sure that if I had been left to my natural pleasantness, I should have judged the tones by a different standard." "Unpleasantness gives the same difficulty as pleasantness." "Nothing in the tones like the bitter, but am trying to equate them." "I don't know what my basis is." "There is some tendency to pucker up my face [with high tones]."

K. "The medium tones are cheerful and pleasing like the sugar." "The low tones have a tiring effect. The high ones are so squeaky they make me a little nervous. They don't seem harmonious and aren't pleasant to hear."

H. "The low and medium tones are like the sugar because they are sweet and pleasant and soothe me. The sharp notes pierce me and annoy me, and stick in my mind more unpleasantly than any. Some of them are so high they seem not to impress me at all."

After the regular experiments were completed, the four practised observers were asked: "What tastes are the following tones most like?" and were given the tones f^3, f^1, and F, each one several times over, with the harmonical. The f^3 was reported like "too strong a sweet" by G, and like sour by the other three observers. The f^1 was reported like sweet by F; like sweet and brown by C; like weak bitter by G; and like weak sweet by Di. The F was reported by F as like bitter and sweet; by C like sweet(?); by G. as bitter and flat; and by Di as 'sickish sweet.' Asked to arrange the feeling-series in order of the difficulty of judgment experienced, F arranged them (easiest first): Depression, Excitement, Strain, Relaxation, Unpleasantness and Pleasantness; C arranged them Depression, Relaxation, Unpleasantness, Excitement and Pleasantness, Strain; G's arrangement was Pleasantness, Strain, Relaxation, Depression, Excitement, Unpleasantness; Di called Strain the hardest, and the others all about equally difficult.

The summary and interpretation of the introspections must be prefaced by a comparison of our conditions with those in the experiments which it was our purpose to supplement. Titchener and Hayes in effect hypothesized the existence of the Wundtian feeling-dimensions. They oriented their observers generally only by single words, designating the qualities to be judged,—arguing that, with the stimuli used, the appropriate feelings should present themselves as bases of judgment, and express themselves in the production of typically different curves. We, on the other hand, have adopted another standpoint; we have taken for granted not Wundt's theory, but his observations. Working with this hypothesis, we have set our observers as definitely as possible within the circle of the experiences themselves, and have tried to ascertain whether or not the Wundtian theory follows. When the Wundtian feelings called for failed to present themselves in the previous experiments, a wide latitude of selection among bases of judgment still appeared possible in the Depression-Excitement and Strain-Relaxation dimensions. In our experiments, if we grant the correctness of the Wundtian observations, under the instruction and repeated suggestion to judge in terms of an empirical standard an observer has no such latitude of selection, but is definitely oriented for a given dimension, should have no difficulty in passing judgments on the basis of that standard, and should give not only typically different curves for the various qualities, but also curves identical in type with those of other observers for the same quality.

But our hypothesis, too, receives no confirmation. To some observers, for some qualities, the tonal and the illustrative experience appeal apparently quite directly in a common aspect, and the judgment is accordingly easy. For other observers, and for other qualities, however, precisely the reverse is true: no common aspect can be directly found and the observer tends, contrary to instructions, to adopt either a more natural standard, or else some highly artificial non-affective standard of his own. Moreover, even when in the Depression-Excitement and Strain-Relaxation dimensions a common aspect is found, it is most frequently designated as pleasantness or unpleasantness, phrased in terms of secondary sensory concomitants, in terms of quasi-attributive character, or in both pleasantness-and-unpleasantness and sensory terms together. The illustrative experiences, that is to say, did not consistently make for ease of judgment. Often, especially in the pleasantness-unpleasantness dimension, exactly the contrary was the case. They did not produce uniformity of attitude among observers, nor bring into play the distinct bases of judgment to be expected. Unlike Titchener and Hayes, we find discriminable differences of attitude not only in the Excitement-Depression and Strain-Relaxation dimensions, but also in the dimension of Pleasantness-Unpleasantness. Why these latter qualities should, with different observers, give different curves, could scarcely have been asked by the earlier investigators, since their question was simply that of the number of typical affective curves for the single observer. In our case, however, this question is especially pertinent. A hint towards explanation is given, perhaps, by the fact that our observers speak of 'sickish,' 'sweet,' 'heavy,' 'warm,' 'cool,' 'big,' 'smooth,' 'localized,' and 'diffuse' pleasantnesses, and of 'bitter,' 'sour' unpleasantnesses, and that they also, after analyzing into sensory components the experiences to which these adjectives apply, speak of 'calming' and 'exciting' pleasantnesses, and of 'straining' and 'irritating' unpleasantnesses. The instruction to judge in the light of definite standards, which were at least partly sensory in character, seems to have disposed our observers to judge, not so much of affective values as of total sense-feeling values.

The question of the basis of judgment, however, cannot be dismissed by a mere reference to the possible diversity of sense-feeling. The fact which we were so particularly concerned to bring out in the first part of our discussion of results, the fact that the curves of the observers in our own and in previous experiments fall into six types, arouses the suspicion that behind this uniformity must lie at least some community of attitude, some few specific sense-feelings, or some few other determining bases of judgment, masked as yet by the apparent heterogeneity of attitudes reported in the more or less incidental introspections. Despite repeated attempts to correlate the six typical curves with single factors or combinations of factors introspectively reported, we are for the present obliged to leave the matter quite unsettled.

We conclude our discussion of results by noting that the curves of our unpractised observers fall into the familiar types and show no distinguishing characteristic save a somewhat irregular course, and that the postponement of the pleasantness-and-unpleasantness series until the last appears to have made no difference to the curves obtained.

Conclusions

(1) We have approached the Wundtian theory of feeling from the standpoint of those determinate experiences which Wundt offers as typical of his three dimensions and six quality-groups. We find no evidence, either in the distribution of judgments or in the observers' introspective reports, for the correctness of the Wundtian theory.

(2) Our curves of distribution of judgments accord with those published by Titchener and Hayes. The whole set of 104 curves falls into three well-marked types of directly opposed courses. (Incidentally we have proposed a new method of measuring the likeness and unlikeness of such curves.)

(3) We have found evidence that the observers' basis of judgment is rather sense-feeling than affection pure and simple. We are not able, however, to correlate the three types of curve with three distinct sense-feelings or with three separable groups of such feelings. Future experiments must show whether this impossibility of correlation is inherent in the method of paired comparisons, as applied to the affective problem, or whether the method may be so refined, on the introspective side, as to furnish a solution of that problem.

We propose now to continue work with metronome-stimuli and systematic introspection. We shall thus be able to take account of the remainder of Titchener's and Hayes' curves, and to determine mathematically if metronome-curves fall into our three familiar types. We hope also, by making the introspective reports systematic rather than incidental, to ascertain the precise bases of the 'affective judgment.'

PRIMITIVE NOTIONS OF THE "SELF"

By Arthur J. Todd, University of Minnesota

I. Anthropologists as well as students of religion have been until recently almost unanimous in pronouncing primitive men anthropomorphic. Especially is this true since the Spencerian view of the origin of the idea of soul and of religion has been coupled up with Tylor's theory of animism as the basis of primitive religion. But much evidence exists and more is constantly coming to light to show that Voltaire's famous aphorism about man conceiving God in his own image, while perhaps true enough of highly developed peoples, is only half true or wholly false when applied to more primitive men. The savage does not feel himself the capstone of creation nor the 'measure of all things.' The idea of his supremacy over the beasts and other men comes perhaps from revealed religion, but it also develops with a growing experience of men and things. At first, however, he conceives the equal if not greater importance of many of the things he sees around him, things which have their own lives, interests, politics, and even fetiches. Notice, for example, the important rôle of animals as Culture-Heroes and as Creators in primitive myth. In other words, if we conceive savage mind as zoomorphic we shall strike nearer the truth. The reason is precisely this: that primitive men have a very confused, ill-defined, hazy notion of their "persons," their "selves."

If it be objected here that savages have such a superabundant sense of personality that they spill it over into the world of nature and work out vast animistic systems upon it, we simply reply that peoples standing low down on the culture-ladder are not strictly animistic, have only a vague, general notion of man's spiritual nature, and if they do project their images into Nature, derive only a nebulous sort of animism which is not really animism at all, but would better be called dynamism, energism, manitouism, or mysticism. It would not be difficult to show that many early travellers, discoverers and missionaries, on whom we must rely for much of our ethnographic material, projected their own preconceived notions of God, soul, spirit, into their interpretation of savage thought. For example, many of our North American Indians who were set down as worshipping a "Great Spirit" had no real concept of a personal spirit. The missionaries caught part of the Indian idea of *manitou*, mysterious causal force, and personalized it as "Great Spirit." We shall offer further illustrations of this point later.

There is perhaps legitimate ground for making the philosophic distinction between personality and individuality. But since individuality develops first in the genetic order we are safe in saying that if the savage has but a vague sense of his physical individuality his notion of his 'person' is still more vague. That his sense of his physical self is vague cannot be doubted. Cuvier somewhere describes the first man as wandering about in ecstacies at the discovery of so many new parts of himself, till he gradually learns that they are not himself but things outside. Perhaps it is even truer that he regards integral parts of himself as things outside. This is a familiar phenomenon among lower animals. The cat plays with her own tail. My dachshund puppy worries his own front leg, bites it, groans, and apparently wonders why it hurts. Certain Euniceae (a group of Annelids) attaining a length of 1.5 metres bite the posterior extremity of their body without seeming in the least to feel it. For this reason apparently these animals voluntarily mutilate themselves when kept in captivity under disagreeable conditions.[1]

The human infant is many months in establishing personal ownership of his fingers and toes. That the savage is often equally backward in identifying himself with his own physical organism and feelings comes out strikingly in an account of Kafir childhood.

> "When a Kafir child has learned this first lesson (fear of fire) he has still much difficulty in recognizing the fact that his pains and aches arise within 'the frame that binds him in.' Take, for example, a headache. One of the most intelligent Kafirs I know told me that he could quite well remember his first headache during childhood. He said that he was conscious that something was wrong somewhere, but did not dream the pain was within his head. The pain might just as well have been in the roof of his hut as in the roof of his head; and it was only when his mother told him that his head was aching that the fact dawned upon him."[2]

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[1] Ribot. Diseases of Personality (Open Court edition), p. 143, quoting Perrier, Colonies Animales. On the general obscurity of the term 'individual' among lower animals, see Ribot, *loc cit.*, 138-9; Espinas, Sociétés animales (2d ed.), Appendix ii; Bergson, Creative Evolution, 259-261.

[2] D. Kidd, Savage Childhood, 61-2. A public school teacher writes me that the same is true of many California children not entirely savage. "I've known," she says, "little chaps just beginning school to have 'an awful sore throat,' and then point to the region of the stomach for evidence."

Similarly the Lower California Indians in Father Baegert's narration could not or did not locate the trouble, when anything ailed them, but called every pain 'headache.' I am inclined to ascribe much of savage disease philosophy, not so much to a belief in evil spirits as causative agents (at least in the beginning), as to original failure to locate the seat of trouble in the sufferer's own person.

Such vague definitions of the physical "self" simply illustrate that lack of sharp dualisms which is the distinctive mark of primitive thought. It is difficult for a twentieth century Anglo-Saxon to seize and realize this primitive monism in all its comprehensiveness and vagueness. Perhaps I can best illustrate it from a symphony concert. If I can conceive myself as listener, music, orchestra, and conductor all in one, or interchangeable at will, then I have at least a glimmering of this phase of primitive mind. To a certain extent it is as guiltless of rigid dualism as the young child's mind, of which Professor Baldwin remarks:

> "Such consciousness seems to lack dualisms. It has no depth nor polarity. It is innocent of the distinction between what is in consciousness and what is external to it (the dualism of "inner and outer"), of the distinction of the subject that thinks and of the thing it thinks about (the dualism of "subject and object"), of the distinction between one thinker and another (the dualism of "self and other-self"—"ego and alter")."[3]

From his experience with the Kafirs, Mr. Kidd concludes: "There seems to be a tendency in the primitive mind to assign internal or subjective agency to phenomena due to external causes, and conversely to attribute external agency to effects which are due to subjective or internal causes."[4] Tylor states as a general principle that "even in healthy waking life, the savage or barbarian has never learnt to make that rigid distinction between subjective and objective, between imagination and reality, to enforce which is one of the main results of scientific education."[5] Major Powell once declared that the confusion of confusions in the minds of non-civilized men is the confusion of objective and subjective. And it is to terms of such psychologic errors and confusions that Professor Lehmann reduces the whole of primitive superstition and magic.[6] M. Lévy-Bruhl in his study of primitive mentality insists that savages do not conform to our logical discriminations between subject and object: "the primitive mind goes farther than merely to *represent* its object to itself: it *possesses* it and is possessed by it. It participates in it not only in the representative but at the same time in the physical and mystical sense of the word. It not only thinks it, it *lives* it." The identification or participation is so transfused with feeling, so imbedded in emotion, that it can hardly be said to be definitely thought.[7] This mystic monism is nothing if not thoroughgoing and consistent. Hence for many savages there is no such thing as a natural set over against a spiritual or supernatural world; neither are there any such mutual exclusions as " waking " and " dream " world, sacred and profane, matter and mind, object-as-such[8] and its properties or qualities.

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[3] J. M. Baldwin, Thoughts and Things, i, 46. [4] Savage Childhood, 62. [5] Primitive Culture, i, 445. [6] Aberglaube und Zauberei, 10, 316, 322, 324 ff., 537, etc.

In such a delightful yet at the same time miserably uncertain Alice-in-Wonderland world anything may happen; all things are possible. And this is precisely why even scientific observers with their concepts and categories make such a mess of interpreting primitive thought. Renan complains somewhere that we are wrong in applying our habitual methods to periods " wherein rivers have sons and mountains give birth." Professor Bain declares that " the leading fact in Belief according to my view of it, is our Primitive Credulity. We begin by believing everything; whatever is, is true."[9] Raise this dictum to the *nth* power and you reach the catholic and elastic quality that stamps primitive mind. How will you classify a mind that believes a man can be born of a rock, or that stones and grass can talk, fire not burn, the dead be dead and alive at once, or that a village is the child of a town?

However it is to be classified, such a mind is not wholly irrational, even though it carries a larger dose of emotion than we deem proper for rational thought. It is thoroughly consistent, at least in the sense that it focusses on a life policy—the will-to-live. It is logical according to its own pragmatic unconscious formulation of logic, viz., a logic of use. For example, the Rev. J. Jette says of the Ten'a that they " have a wonderful faculty for believing or disbelieving what they choose. Their intellect seems to be altogether at their will's command, ready to give or refuse its assent according to the direction which it receives from the will. They never judge of the credibility of a report or statement on the merits of the case. . . . The ultimate reason which can be detected in almost every instance that really determines their belief is: what benefit shall accrue to me from such a belief?" [10] We must remember that not only the Ten'a but also civilized men, all of us, see not with our eyes, and hear not with our ears: we see with what we have already seen and felt; we hear what we have heard and felt before.

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[7] Lèvy-Bruhl, Les fonctions mentales dans les sociétés inférieures, pp. 426, 67, 39, etc.

[8] For example, among Australians: Spencer and Gillen (Native Tribes of Central Australia, 697), speak of certain geometric drawings on their *churinga*. " Thus for example a spiral or a series of concentric circles cut on the surface of a certain *churinga* will designate a *nyassa* (gum-tree); but an exactly similar design cut on another *churinga* will represent a frog." Cf. Parkinson, Dreissig Jahre in der Südsee, 621-7, 234-5. This same phenomenon may frequently be observed in children's games and drawings.

[9] The Emotions and the Will, 511.

We need not be surprised, then, to find these uncritical minds rejecting such trifling distinctions as that between belief and knowledge. Such a mind does not *believe in*, say, ghosts or dreams or metamorphoses: it *knows* them; they are perfectly valid, normal experience. Beliefs and superstitions arise only when experience is questioned, when categories of natural, supernatural, etc., are set up. But at first all is natural. Hence we must not be unprepared to find the savage defining his " self " in terms of his name, his shadow, his soul, his feces, exuvia, property, family, clan-group, or other, to us, incongruous things and relations. For all these are *natural* parts of his person in his conception of all as natural. Furthermore, the principle of *totum ex parte* is a fundamental principle of primitive thought. Applied to the concept of self, it has been stated thus: the personality of a being is indivisible and resides in its entirety in each of its parts.[11] Hence the extreme solicitude of certain tribes for the clippings of their hair, parings of nails, feces, etc. Hence also such customs as the secret name or the taboo on the shadow. With these general principles in mind let us examine a few illustrations of the various elements which enter into the savage's concept of his self.

II. The name has been almost universally conceived as part of the self. " For all practical purposes the Kafir thinks the name is the man." [12] The Eskimo of Angmagsalik say that man consists of three parts, the body, the soul, and the name (atekata). The last enters the child when after its birth a sort of baptism is performed by rubbing water on its mouth and naming the name of the dead after whom the child is to be called. When a man dies the *atekata* remains with the body in the water, or in the earth until a child is named after him. It goes then into the child and there continues its existence.[13] Sayce says of the Babylonians that name-giving was an important event in the child's life. "Like other nations of antiquity the Babylonians conformed the name with the person who bore it; it not only represented him, but in a sense was actually himself." A corresponding belief is hinted in the Chaldean oracle, "Never change native names."[14] Closely connected with the name as self is the common belief in the 'power of the word' and the word as a person.[15]

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[10] *Journal of the Royal Anthropological Institute* (hereafter abbreviated as *JAI*) xxxvii, 158-9.

[11] Hubert and Mauss, Esquisse d'une théorie générale de la magie, *Année Sociologique*, vii, 62. Lippert in his discussion of fetichism, especially the Royal Person as Fetich, and the statue of the god or king as fetich, states the same general principle. (Kulturgeschichte, ii, 438, 466 ff.)

[12] Kidd, Savage Childhood, 72.

The shadow as part of the self is almost equally common. "It strikes one as strange at first," says Miss Kingsley of the West African negroes,[16] "to see men who have been walking, say, through forest or grass land on a blazing hot morning quite happily, on arrival at a piece of clear ground or a village square, most carefully go around it, not across, and you will soon notice that they only do this at noontime, and learn that they fear losing their shadow. I asked some Bakiwari I once came across who were particularly careful in this matter, why they were not anxious about losing their shadows when night came down and they disappeared in the surrounding darkness, and was told that that was all right, because at night all shadows lay down in the shadow of the Great God, and so got stronger. Had I not seen how strong and long a shadow, be it of man or tree or of some great mountain itself, was in the early morning time? . . . Murders are sometimes committed by secretly driving a nail or knife into a man's shadow, and so on;

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[13] Hartland, Primitive Paternity, i, 218, after von Adrian and Holm.

[14] Babylonians and Assyrians, 44. Similar evidence from other peoples: Mooney, *7th Bur. Ethnology*, 343, 352; Ellis, Ewe-Speaking Peoples, 98-9; Rivers, The Todas, 627; Chamberlain, Things Japanese, 4th ed. 344; Spencer and Gillen, *N. T. C. A.*, 227; Hill Tout, xxxv *JAI*, 152; *Jour. Asiat. Soc. of Bengal*, iii, 20; DeGroot, Religious Systems of China, i, 212; Levy-Bruhl, Fonctions mentales, etc., 45 ff.

[15] Certain taboos on words, secret languages, use of formal prayers, rituals in archaic languages, inscriptions, spells, benedictions, curses, formulas, incantations, common in primitive life are expressions of the old belief that the description or mention of an act suffices both to produce it and its effect. "Speech if uttered in a whisper is breath, if spoken aloud it is body," said the Upanishads. (Sacred Books of the East, i, 231). Survivals of this ancient philosophy occur in contemporary religion. A Jesuit writer in *Contemporary Review* for January 1897 says: "Sacramental words according to Catholic doctrine, are words of power." Lafcadio Hearn has left a curious instance of the power of the word as a person. In old Japan the dragon-character once became an actual dragon. A man who had ridiculed the form of a certain character, likening it to a 'swaggering wrestler' awoke in the night under a terrible pummeling, only to find that the pummeler was the letter he had laughed at. A similar episode centers about a man who laughed at the rice-character. (Glimpses of Unfamiliar Japan, i, 32-3).

[16] West African Studies, 176.

but if the murderer be caught red-handed at it, he or she would be forthwith killed, for all diseases arising from the shadow soul are incurable. No man's shadow is like that of his own brother, says the proverb." A high-caste Brahmin becomes unclean if the shadow of a man of lower caste falls upon him. Chinese burial ceremonials noted by De Groot illustrate the same sentiments. "When the lid is about to be placed on the coffin, most of the by-standers not belonging to the nearest kindred retire a few steps, or even make off for the side apartments, as it is dangerous to health and detrimental to good luck to have one's shadow enclosed in a coffin. . . ." "They now lower the coffin into the grave, the principal mourners wailing and stamping their feet an unlimited number of times." Most of the by-standers recoil a few paces lest their shadows should fall into the grave and harm thus be done to their persons. . . . The geomancer and his assistants are wise enough to stand on the side of the grave which is turned away from the sun."[17] "A Kafir cannot always distinguish between himself and his shadow. Thus he is angry when a man—or even a child—stands on his shadow, for it is much the same thing as standing on his body. It will be observed that when a native approaches a number of men who are sitting down, he is careful to avoid treading on their shadows, or even tries to prevent his long shadow being seen in comparison with their short ones. This is especially the case with inferiors approaching their betters. . . . It is thought that a sick man's shadow dwindles in intensity when he is about to die, for it has such an intimate relation to the man that it suffers with him. . . . Native doctors apply medicine to people's shadows as well as to their bodies. . . . The shadow of a tree is said to feel the touch of the man's feet. And if the shadow of a tree is regarded as an organic part of the tree, how much more must the shadow of a man be considered to be a part of a man's personality."[18]

European folklore and mediaeval poetry reflect the popular superstitions regarding the shadowless man. Chamisso in his famous tale *Peter Schlemil* exploits these ideas. Schlemil barters his shadow to the devil for a magic purse of gold. He becomes forthwith an outcast through popular suspicion and fear of the shadowless man. He finds his shadow running loose one day in the woods, pursues and catches it, but is later compelled by the devil to relinquish it. Henceforth he must go without it for good and all. Cruikshank's illustrations show how a shadow may be detached, folded up or chased in quite the primitive fashion, and add strikingly to enforce the concept of the material substantiality of the shadow. (See the illustrated edition in Putnam's Ariel Booklets Series.) The play of present-day American school children indicates that these old ideas still survive in dramatic vigor. Many a child has been thrown into a paroxysm of fear and crying because a bullying comrade brutally stepped on his shadow.

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[17] Religious Systems of China, i, 94, 210, 211.

[18] Kidd, Savage Childhood, 68, 70, 71.

Again, the image, "likeness" or picture, are identified with the self. To quote once more Mr. Kidd: "The raw Kafir has, as a rule, the greatest objection to having his photograph taken. He considers his 'likeness,' as he calls it, a part of his personality."[19] I have encountered the same belief among certain Indians of Western Nevada. I asked the women to allow me to photograph themselves with their babies, but was refused, from fear that I would take away part of their "selves" in my little black box: by the principle of *totum ex parte* I might work magic on the part and hence injure the whole of their selves. I was more fortunate than some students in America and other parts of the world in not having my camera smashed for my pains. More than once Catlin had much difficulty in clearing himself of the charge of evil magic while painting his North American Indians, all on account of this notion of the image-self. The Mandans, he wrote, "pronounced me the greatest *medicine-man* in the world, for they said I had made *living beings,*—they said they could see their chiefs alive in two places—those that I had made were a *little* alive—they could see their eyes move—could see them smile and laugh, and that if they could laugh they could certainly speak, if they should try, and they must therefore have *some life* in them. The squaws generally agreed, that they had discovered life enough in them to render my *medicine* too great for the Mandans; saying that such an operation could not be performed without taking away from the original something of his existence, which I put in the picture, and they could see it move, could see it stir."[20]

Survivals of this old belief in the image-self occur in certain modern religious customs—the icon; miracle-working pictures of the Virgin, of Christ, of the saints; medals, scapularies, images; and until recently processions with the banner of the Virgin and holy relics against siege and pestilence. In other words modern fetichism goes back to this old vague mystic definition of the "self." M. Lévy-Bruhl after citing a long list of ethnographic observations on the image-self, dismisses the notion that it is a result of puerile confidence in analogy, or of mental weakness and confusion, or of a naïve generalization from some animistic hypothesis. To the contrary, it is the result of *really perceiving* the object (*i. e.*, the self) in terms different from ours.[21] De Groot finds the basis for such association of images with beings in suggestive shapes, analogous forms. "An image, especially if pictorial or sculptured, and thus approaching close to the reality, is an *alter ego* of the living reality, an abode of its soul, nay it is that reality itself."[22] From Catlin's observations we gather that at least part of the ascription of life to a portrait is referable to the optical illusion of its moving eyes. Hence the image-self is a real experience and not mere juggling with or "fudging" experience. It is warm and intimate reality, not wilful nor fantastic self-delusion.

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[19] *Op. cit.*, 71.

[20] North American Indians, i, 107-8.

[21] "*Si les primitifs perçoivent l 'image autrement que nous, c'est parce qu'ils perçoivent aussi autrement le modèle.*" (Les fonctions mentales, etc., 44.)

[22] Rel. Syst. of China, iv, 340.

In taking up the soul as part of the self we enter debatable land. I do not believe it is necessary for our purposes to plough extensively over this ground, except to remark that primitive men have no uniform concept of the soul. Its size, shape, number, functions and destiny vary from tribe to tribe. The origin of the concept of soul is likewise probably far from uniform. I have long questioned the all-sufficiency of the pathological swoon-and-dream theory. Nor has M. Durkheim's caustic critique of the Spencer-Tylor animism strengthened my faith in the older theory. Yet I have no substitute to propose except by way of suggesting that sufficient attention has not been given to certain normal psychologic processes from which the idea of a soul might easily have arisen. The common phenomenon of *auditory type of perception* and the inner voice that accompanies conscious thinking would seem to furnish materials readiest to hand for constructing the concept of a little man inside of us, of a guardian spirit, of a 'double,' of a 'soul.' When I (say in the character of an Australian black-fellow or Caroline Islander) think and hear the words of my thought ringing inside my brain-pan, what more natural than to conclude that this still small voice is a mystic somebody at once me and still different from my hands, my shadow, my property, and other parts of my "me?" This conjecture is at least plausible and has the merit of not attempting to derive the normal from the pathological. I should, however, by no means reject pathology as one probable factor in the evolution of the idea of the soul-self. Ribot cites a case from Dr. Leuret which shows how the notion of multiple souls might have arisen. A man convalescing from a fever "believed he consisted of two individuals, of which one was in bed, while the other walked about. Although without appetite, he ate a great deal, having, as he said, two bodies to feed."[23]

Yet such discrimination could only occur where self-consciousness had attained considerable development. Hence we should not expect it from low grade peoples. The same might be said of the dream-self. I should offer the suggestion that instead of the notion of the 'double' originating in dreams, the dream-double would never have crossed the threshold of the savage's attention and interest until the concept of the double had already become pretty clearly defined. Furthermore, we must avoid allotting any special significance to primitive dream-life. For, as we have already seen, the savage makes no distinction between dream and waking life; both coalesce and both are equally valid.

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[23] The Diseases of Personality, 34-5.

But whatever the origin of the concept of soul, there is no questioning the identification of the soul or the dream-double as real parts of the self. The familiar reference of sickness or disease to mishaps suffered by the soul clearly indicates this identification. Some one steals my soul; I sicken; the medicine man hunts out the thief, recovers it and restores it to my " me; " this re-integration of my " self " heals me. My dream double goes wandering off o'nights and loses its way back; I sicken from the subtraction; the medicine man once more goes hunting and finds the errant part of my " me; " once more I am restored.[24] The important point here is the vital unity of the self even when it has been differentiated into a multiplicity of parts.

One of the most fascinating developments of the savage sense of personality is the identification of property as part of the self. A number of interesting problems cluster about the primitive attitude toward property. Is there such a thing as a sense-of-property? Why was the property of the dead destroyed or alienated? How does the distinction between real and personal property originate? If property is identified with self how shall we account for that state of economic communism which is well-nigh universal in rudimentary societies, and what does it argue about the concept of self? Our discussion must be limited to only a summary treatment of these problems.

In the first place, man as an object of natural history has incontestably an instinct for property; yet in the same sense that the amoeba has an instinct for property, and only in that sense. This instinct is simply the expression of the more general and fundamental will-to-live. But if we observe man as a member of human society the matter is by no means so clear; property here seems not to be rooted in mere love of possession. For we find the instinct modified, contravened, and even obliterated by other instincts and motives. What seems to be man's 'instinct for ownership' may frequently turn out to be in reality love of activity or of mastery. And the reason lies in just this fact, that he is member of a group, and that his self, which extends itself more or less consciously to property, takes on the color of his group surroundings and is modified by them.[25] Man in society is never fundamentally and inalterably egoistic, but is at once self and other-self, ego and alter, inextricably blended and interwoven. To forget this is to fall into a maze of fallacies surrounding the ancient problem of the relation of individual to society. The moment man attains *self*-consciousness he has emerged from natural history with its brute struggle for self-maintenance and its crass sense of property, and has become the center of those psychic and social forces which really create him *man*. "As a herd of individuals mankind would have a natural history as other animals have; but personality can only emerge out of intercourse with persons."[26] This intercourse with persons gives him in the most absolute sense his "me," including its expression in property and every other form. It is quite natural that with the attainment of consciousness of self, man should seek to enlarge his self-concept, to define it, in other words, to develop his individuality. But this does not necessarily mean to aggrandize his self at the expense of other selves in the group. The constant check upon any such impulse to aggrandize is furnished by the group consciousness of its unity. Consequently the individual self is subordinated to the interests of the group and takes its key and coloring from it. This is of course the secret of the tremendous power of the mores, traditions, folkways, and all that group of more or less unformulated means for social control, to say nothing of more highly developed institutions for control. It would seem then that the original biologic sense of property in its individualistic aspect may become and does become something quite different when the sense of self-as-a-social-being emerges. It is not necessary to state dogmatically that the more primitive a group, the more communistic it is, nor that communism in property was ever absolute and uncompromising. It is sufficient to find communism in land, mines, food, etc., in every quarter of the globe to conclude that such men manifest an undeveloped non-militant sense of personality in property.[27] It goes to show that property as we have come to consider it is either an acquired characteristic (and as such modifiable), or a reversion to the crude sub-human instinct for self-maintenance at any cost.

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[24] Many Old Testament references to the soul, quite apart from any mere verbal suggestion, appear to be pretty nearly upon this plane of thought. "He *restoreth* my soul," (Ps. 23); "The liberal soul shall be made *fat,*" (Prov. 11.); etc. And later theology involving destruction of both soul and body sounds curiously like primitive beliefs about the "second death" of a material substance.

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[25] Take, for example, the matter of property in shelter. According to Fewkes (*Smithsonian Report*, 1910, p. 614), what led man originally to seek caves for habitation was "a desire for shelter from the elements, but not so much protection for himself as for others—for his offspring. . . . Their use for habitation was secondary, the primary motive being mainly altruistic, the same as that which leads the insect, bird, and mammal to make their nests." It is interesting to note that caves were likewise used for other distinctively *social* and *communal* purposes: burial, storing sacred paraphernalia, performance of sacred rites, etc.

[26] James Ward, *Encycl. Brit.*, 9th ed., vol. xx, p. 75, note.

On the other hand, since men are marked off from each other and do not coalesce in a gelatinoid mass, it is perfectly natural and altogether in keeping with group interests that their selves should tend to expression in purely *personal* property. Hence we find even in communistic groups private property in clothes, ornaments, trinkets or tools; and taboos to preserve their individual ownership. Thus the taboo is frequently the outward visible sign of the participation of such property in the owner's personality; hence its sacredness; hence its inviolability. This, too, probably explains at least in part the wide-spread custom of destroying the property of the dead. Such a principle eliminates many of the fantastic interpretations of the custom, which really explain nothing, and themselves require interpretation upon interpretation. Such, for example, the notion that a man's horse or house or widow or slave must be destroyed because of the dead person's covetousness or jealousy;[28] or that by smashing or burning a man's pottery, bows, spears, etc., their "souls" are released and enabled to accompany their late owner. These reasons may be given by savages who have forgotten the original significance of the custom. The more fundamental notion seems to be that property of the dead is not destroyed to *benefit him,* but *because it is he himself,* literally and absolutely. M. Lévy-Bruhl[29] applies the same reasoning to the taboos on widows between the 'first' and 'second' deaths of their spouses, though I confess that the principle of ghost avoidance may be in this instance nearer the truth. The finesse displayed by modern Kafirs brings this identification of the dead man's property with his self into clear relief, and seems to justify our interpretation of mortuary destruction of property.

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[27] "If the savage is incapable of conceiving the idea of individual possession of objects not incorporated with his person, it is because he has no conception of his individuality as distinct from the consanguine group in which he lives." (Lafargue, Evolution of Property, 18.) Cf. T. E. C. Leslie's introduction to Laveleye's Primitive Property, pp. xi-xii: "Property in the infancy of social progress consisted, one may say, simply in the feeling of unity and consequently co-ownership on the part of the men of a tribe, horde, clan, sect, or family."

[28] This does not deny that taboos and destruction of property of the dead are part of the general cult of avoidance and propitiation of ghosts and daemons.

[29] Les fonctions mentales, etc., 378 ff.

> "If a Kafir should buy a blanket and coat and never use it before he died, then it would not be buried with him but would be passed on to the heir; if the man had worn it but once, and had soiled it with a little perspiration, then it would be buried with him. In this latter case it contains a part of his dirt and therefore a part of his personality. So with an assegai; the only part of the weapon that has the man's dirt ingrained into it is the wooden handle; this therefore contains a part of the man's personality and must be buried with him; but the iron point does not come into contact with the man, and so contains no part of the man's personality; therefore it needs but a ceremonial washing to make it the property of the heir."[30]

Such a concept of the dirt-property-self marches along with and is perhaps a variant of the notion of the feces as part of the personality. Its connection with exuvial customs (preserving parings of nails, clippings of hair, etc.) is even more striking. The exuvial-self explains many exuvial sacrifices and exuvial fetiches.

We have already hinted several times in passing that the individual's sense of his own personal self is frequently subordinated to a mystical sense of the group personality, the larger self. We shall now see more precisely how this group sense acts upon the individual's definition of his self. To treat the matter thoroughly would require an elaborate review of the whole subject of primitive methods of reckoning kinship and relationship. We must confine ourselves, however, to bare indications of how these matters were conceived and felt. Primitive kinship, so it appears, rests originally on common work, common ownership, common food: in other words, on participation in common group activities and supplies. Blood relationship as we know it is certainly a derivative from this earlier (to us), conventional method of reckoning kinship. Two lines of proof exist: first, the reckoning of relationship outside the bounds of consanguinity; second, the ignorance of the process of human reproduction still to be found among certain tribes. Such institutions as the couvade and adoption are fictive devices indicating a transition period between an earlier vague definition of kinship and a more precise physiological concept of relationship.[31] The general historic sequence of ideas concerning relationship is, first, that a child is related to his group, next to his mother and her kin but not to his father, next to his father and his kin but not to his mother, and finally to both father and mother.[32]

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[30] Kidd, Savage Childhood, 67.

[31] For a detailed exposition of these points see the writer's Primitive Family as an Educational Agency, chap. iv.

All these variations in the definition of kinship are at the same time variations in the concept of personality. We are interested particularly in that concept of kinship and of personality by which the individual is subordinated to his family or larger social group. It is immaterial to our purpose whether such a concept involves group-marriage or not. Nor are we required to accept such a literal view of 'collective parenthood' as Professor Kohler sets forth.[33] Yet we must not be too hasty in rejecting such ideas as fantastic, for the human mind is infinitely credulous. Take, for example, the Kafir's notion of his group-self. "A Kafir feels that the 'frame that binds him in' extends to the clan. The sense of solidarity of the family in Europe is thin and feeble compared to the full-blooded sense of corporate union of the Kafir clan. The claims of the clan entirely swamp the rights of the individual. . . . The striking thing about this unity of the clan is that it was not a *thought-out* plan imposed from without by legislation upon an unwilling people, but it was a *felt-out* plan which arose spontaneously along the line of least resistance. If one member of the clan suffered all the members suffered, not in sentimental phraseology, but in real fact."[34] This intimate sense of group solidarity becomes translated into terms of individual personality; so we find that every true Kafir has two personalities: his *idhlozi* or individual, personal, inalienable spirit; and his *itongo* or ancestral spirit, which is not personal but tribal, and comes not by birth but by initiatory rites. To be without this clan or tribal spirit is the greatest calamity a Kafir can conceive. Such a man goes through life 'unprotected;' that is, his ancestral guardian angel is lacking, his self is dwarfed and truncated. In British Nigeria we are told that the individual

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[32] This is a modified expression of Lubbock's much debated dictum, in his Origin of Civilization, 3d ed. p. 149; repeated in Marriage, Totemism and Religion (1911), p. 52.

[33] Kohler in defending Morgan's theory of group marriage deduces the common occurrence of group kinship from primeval collectivity of women. Hence for the primitive man the group of women whom he calls 'mothers' form a real individual, a single personality endowed with a plurality of genital organs. Hence it is not surprising that he considered himself peculiarly related to this multiple personality. All of which would mean also that every child is *ours* to the collective-mother. (*Zts. f. Rechtswissenschaft,* xix, 171-188, 423-432; xxi, 252-267.

[34] Kidd, Savage Childhood, 15.

"is sunk in the family, village, or tribe."[35] Of our American Indians Bandelier says: "It may be said of the red man that he keeps secrets in the same manner that he lives,—namely in groups or clusters. The reason is that with him individualism, or the mental and moral independence of the individual, has not attained the high degree of development which prevails among white races."[36]

The reasons for such subordination of the individual in both his actions and his thought of himself are to be found in the exigencies of self-maintenance and self-perpetuation, which for their successful issue have developed a closely woven fabric of community life. These conditions together with the ignorance and inexperience of primitive mind gave rise to that vague and unformulated philosophy of the self and men and things which might easily be called secular mysticism.[37] It is interesting to note that when this vague mysticism becomes differentiated into Sacred and Secular, the Here and the Beyond, and gods arise, these gods are at first *group* gods, not personal gods. The primitive Semites, for example, conceived their gods as caring only for the tribe and not for the individual.[38]

Such group solidarity appears all the more strikingly in primitive notions of sin and its punishment. We must remember that primitive sin was wholly an objective or ritualistic breach, and not a sense of ethical shortcoming. Precisely because it was objective and because of the close-knit life of the group in other respects, the sense of sin became, too, a group-sense. Hence the breach of any member of the group involved the whole. Ten righteous men might have saved Sodom, but the un-rightness of one would have sufficed to bring down its destruction. This sense of group responsibility is the source of a tremendous sanction for discipline and right conduct. It gives to taboos their inviolability.

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[35] Mockler-Ferryman, British Nigeria, 230.

[36] The Delight Makers, 13. Mr. E. J. Payne in his profound study of American aboriginal language discovered that the collective or we-form is the more common and the selective or I-form exceptional. "No more interesting illustration could be adduced of the sense of solidarity naturally pervading the food-group, and of the natural sense of weakness in its members individually." (Hist. of America, ii, 188.)

[37] "The primitive man can hardly have been definitely conscious of values which were not supported and shared by the group of which he was a part. A direct result of such a condition would be a vague, indefinite sense of his own personality. The group itself will not be analyzed, but will be conceived in the gross, as the universe in which he moves and has his being, as, in fact, identical with *himself*." King, Development of Religion, 66. This predominance of the group and group-feelings due to primitive notions of 'participation,' seems to be what James had in mind when he wrote (Prin. of Psychology, ii, 368, note), "In general it is probable that the consciousness of how we stand with other people occupies a relatively larger part of the mind, the lower one goes in the scale of culture."

[38] W. R. Smith, Religion of the Semites. New ed. (1901), pp. 258 ff. This seems to be the religious expression of 'pessimistic evolutionism'; recall, for example, Tennyson's *In Memoriam*, lv:

"Are God and Nature then at strife,
That Nature lends such evil dreams?
So careful of the type she seems,
So careless of the single life."

The story of a three years' famine in Israel during the reign of David illustrates these points. (ii Samuel, ch. xxi.) Saul had slain certain Gibeonites contrary to treaty. Years afterward the famine ravaged. David enquired of the Lord. The Lord explained the affliction of the nation as resulting from " Saul and his bloody house, because he slew the Gibeonites." The Gibeonites demanded blood-revenge—the hanging of Saul's seven sons. This was granted, and the seven were hanged "in the hill before the Lord." Evidently this wiping out of seven guilty individuals squared the accounts of the whole group and pleased the tribal god, for we are left with the impression that all went well afterwards and that the famine was raised. Similarly, violation of a taboo by an Eskimo brings punishment upon the whole group. An Eskimo hunter must avoid contact with corpses or bleeding persons. For the souls of sea animals are quick to detect these contaminations and evade the hunters. If a hunter has had the ill luck to do either of these things he must at once avow it. "If he does not do so he will bring ill luck to all the hunters. . . . The transgressor of a custom is distasteful to Sedna and to the animals, and those who abide with him will become equally distasteful through contact with him. For this reason it has come to be an act required by custom and morals to confess any and every transgression of a taboo, in order to protect the community from the evil influences of contact with the evil-doer. The descriptions of Eskimo life given by many observers contain records of starvation which, according to the belief of the natives, was brought about by some one transgressing a law and not announcing what he had done."[39] Death, according to Thonga belief, is both cause and result of group contamination; and a village in which it occurs cannot come back to the ordinary course of life without a special collective purification.[40] The Colonial Puritans had this group sense of sin highly developed. In the early 18th century we find Judge Sewall, one of the witch-baiters, making his confession of penitence for his judicial acts in that unhallowed business; he asks for the prayers of the brethren that God may "not visit the sin of him or of any other, upon himself, or any of his, nor upon the land."[41]

This subordination of the individual's sense of self to that of the group is still further illustrated by certain primitive beliefs regarding reincarnation, family-totems, etc. The Egyptian Book of the Dead speaks of a period "when Horus came to light *in his children*."[42] A Maidu legend (California) tells of the miraculous birth of Oan-koi-tu-peh, whose sire was the Red Cloud. The Red Cloud told the young mother: "Whenever you see him think of me. This boy has no life apart from me; he is myself."[43] An Irish manuscript of the 11th century relates a similar notion regarding the great hero Cuchulainn. The men of Ulster took counsel about him, fearing his early death; they "wished to give him a wife that he might leave an heir, *for they knew that his re-birth would be of himself*."[44] The laws of Manu express the same belief: "The husband, after conception by his wife, becomes an embryo and is born again of her."[45] A corresponding Hindu ritual required the father to address a new-born babe thus: "From limb by limb thou art produced; out of the heart thou art born; thou indeed art the self (âtman) called son."[46] Among the Aruntas of Australia every individual is a direct reincarnation of an ancestor of the mythical alcheringa period or of some totemic animal of that period.[47] I have reserved as a final illustration of the identification of the individual with his family some remarkable cases from northern China. Mr. Johnston, who relates these incidents, encountered them as actual problems in the course of his daily affairs as a judge in China:

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[39] Boas, *Pop. Sci. Monthly*, 57: 627; cf. in general, Lippert, Kulturgeschichte, ii, 497; F. Justi, Geschichte des alten Persiens, 199.

[40] Junod, The Life of a South African Tribe, vol. i, p. 152.

[41] Weeden, Economic and Social History of New England, i, 421-2.

> "The Weihaiwei farmer has indeed so limited a conception of his own existence as a separate and distinct personality that in ordinary speech he continually confuses himself with his ancestors or with living members of his family. Examples of this are of repeated occurrence in the law-courts. "I bought this land and now the Tung family is trying to steal it from me," complains a petitioner. "When did you buy it?" asks the magistrate. "Two hundred years ago," promptly replies the oppressed one. Says another, "My rights to the property of Sung Lien-têng are being contested by my distant cousin. I am the rightful owner. I buried Sung Lien-têng and have charge of his soul-tablet and carry out the ancestral ceremonies." "When

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[42] Book of the Dead, c. 112, transl. Renouf, in *Proc. Soc. Bibl. Arch.*, xvii, 8; Budge's translation of this chapter renders the idea thus: "Then the company of the gods, who were among the divine followers of Horus when he existed in the form of his own child, etc. . . .

[43] S. Powers, Tribes of California, *Contrib. to N. A. Ethnology*, iii, 299.

[44] Kuno Meyer, *Archeol. Rev.* i, 70.

[45] Sacred Books of the East, xxv, 329; xii, 334. An echo of this oriental belief perhaps occurs in Nicodemus' question about rebirth.

[46] Sacred B. of the E. xxx, 211; cf. a passage in the Brihadâranyaka-Upanishad (Sacr. Books, xv, 96).

[47] Spencer & Gillen, *N. T. C. A.*, 202-4, 169-70.

did Sung Lien-têng die?" questions the magistrate. "In the fortieth year of K'ang Hsi," is the reply. This means that the deceased whose property is in dispute died childless in 1701, that plaintiff's ancestor in that year defrayed the funeral expenses and acted as chief mourner, that by family agreement he was installed as adopted son to the deceased and heir to his property, and that plaintiff claims to be the adopted son's descendant and heir. Looking upon his family, dead and alive, as one and indivisible, he could not see any practical difference between the statement that certain funeral rites had been carried out by himself and the statement that they had been carried out by a direct ancestor. . . Another litigant, whose long residence abroad had had no apparent effect on his general outlook on life, came to me very recently with the complaint that on his return from Manchuria he had found his land in the possession of a neighbor. "I went to Manchuria as my family had not enough to eat," he said. "I came home this year and wished to redeem the land I had mortgaged before I went away. But I found it had already been redeemed by my neighbor, a cousin, and he refuses to let me redeem it from him." On being asked when he had mortgaged his land and emigrated, he replied: "In Chia Ch'ing 3"—that is, in 1798. He was merely identifying himself with his own great-grandfather. . . . In another case a man whom I will call A brought a plaint to the effect that he wished to adopt B, and that C for various reasons refused to allow this adoption to take place. On investigation it turns out that B is dead and that it is his infant son D whom A really wishes to adopt. B and D—father and son—seem to A merely different expressions, as it were, of the same entity. This does not mean, of course, that supposing B were still alive it would not matter whether B or D actually became A's adopted son. The rules of adoption in China are strictly regulated. A man cannot adopt any one he likes. Not to mention other necessary conditions, the person adopted must belong to the appropriate generation, that is, to the gneration immediately junior to that of the adopter. In the case before us the infant D belonged to the proper generation, and his father B could not have been adopted. To our notions it seems all the stranger that A, knowing this, should have spoken of B when he meant D; yet this manner of speech is exceedingly common." The writer then goes on to show further evidence that the individual is not regarded as an independent unit, citing the restrictions placed on the powers of the individual to dispose of real property.[48]

At this point it may be of interest to note that mere birth does not always confer personality upon the infant. Frequently it must be achieved through rites and ceremonies. Thus among the Bayaka a boy has no personality distinct from his father's until circumcision.[49] In British Central

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[48] R. F. Johnston, Lion and Dragon in Northern China, 139-41. Added light is shed upon such phenomena by Mr. Tao's explanation of ancestor worship. It did not originate, he claims, "from the dread of ghosts, nor is it an 'animistic lottery' for securing material welfare or advantage. Ancestor worship is rather the expression of an instinctive craving to trace the origin of the self." *Sociological Review,* vi, 51.

[49] Torday and Joyce, xxxvi *JAI,* 42.

Africa a new-born infant is not regarded as an individual, as a person, until it receives at least a portion of its mother's personality in the shape of nourishment.[50] Among the Giliaks of Saghalin Island new-born children for some time are not differentiated into 'boys' and 'girls.'[51] Among the Zuñi Indians Cushing tells us that "when a Zuñi woman is about to give birth she is, if possible, retired into a room of "sacred enclosure." The entrance and windows of this room are carefully screened with blankets, and over or on the entrance way, whether door or sky hole, a plume of warning, or taboo, is attached so that none but the appointed may enter. Here the child must remain until the morning of the tenth day after birth. For as yet it is not born into the world of men. Nine days, representing the months of its gestation, are required for its formation and "hardening" as a human being, against all malign influences. For it is supposed by them that during the period of this formation, the child is, as were all beings of creation when the world was new, *kyai-u-na*—unripe and susceptible, impressionable even as are the grains of growing corn when in milk" . . .[52] Many other curious birth customs probably owe their origin to similar beliefs. Collateral evidence of the child's supposed lack of personality may be deduced from primitive infanticide, child sacrifice, and child cannibalism. Another line of evidence is suggested from West Africa, where death is usually considered to result from sorcery and where sorcerers are hunted out and killed: it is significant that no such pursuits follow the death of a child, a slave, or a person of no consequence.[53]

III. We have seen how according to primitive philosophy a man acquires his self, and how he may lose part or all of it. Now we must turn to observe how he can change it. In brief, we shall examine the doctrine of metamorphosis in its relation to the concept of self. This will necessitate a momentary return to the savage's general philosophy of nature. We suggested a while ago that this philosophy was altogether too vague to be termed definitely animistic, and suggested mysticism, dynamism, etc. The truth is that to the primitive mind the world and its events were fluid, changeable, capricious, not to be caught and held in fixed and hard categories.

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[50] Werner, British Central Africa, 103.

[51] Piludski, *Anthropos*, v, 756-74.

[52] Primitive Motherhood, 30-1. (In *Proc. 1st Cong. of Mothers*, Washington, 1897.)

[53] Levy-Bruhl, *op. cit.*, 325, after Nassau.

The only universal laws were, first, that all is possible; second, that all is related; third, that all changes.

The first 'law' is too obvious to require illustration. The second, the law of relation, or participation (mystic symbiosis, as M. Lévy-Bruhl puts it), is not a definitely reasoned pantheism, but a vague emotional feeling of community, based upon a failure to mark off the various kingdoms and forces of nature.[54] Here fusion and identity, and not difference or separation are significant. Men and all other things swim in a sort of 'imponderable ether, that fills all space, rigid as adamant, but infinitely elastic,' etc. Such a vague comprehensive concept gives the basis for and expresses itself in various forms of magic, divination and religion: such as fertility rites, food-increase dances, magical communication of powers and qualities by contact. In terms of personality the concept finds its expression as the human self in direct contact with mysterious impersonal force, permeated by it, communicating it. Hence we get the notion of a person as 'mana,' 'orenda,' 'wakanda' or 'manito;' that is, as emanating or manifesting a peculiar quality or power. Thus chiefs, strong men, certain plants, animals, inanimate objects or natural forces are said to have *mana*. This must not be confused with 'possession' by familiar spirits or geniuses. It is rather a manifestation of vague pervasive vital force. The words denoting it are used sometimes as adjectives, sometimes as substantives, and may be rendered as power, strength, mystic, terrible, sacred, secret, remarkable.[55] It is all of these and more. Like many other primitive concepts this almost baffles attempts to translate it into modern logical thought or speech. But this much is certain: it denotes a vital pervasive compelling force in primitive thought. And the phenomenon is so widespread that one is almost justified in pronouncing it a universal stage in the evolution of human thought, a stage anterior even to the soul or double. Observers have noted it throughout Indian America, in Melanesia, among the Maori, in Africa.[56] The Fêng-Shui of the Chinese and the Virtue of classic antiquity and primitive Christianity are survivals of the same thought.[57] This is not exactly fetichism in the modern sense of fetich as the dwelling place of a definite spirit, but it is no doubt the broad base from which fetichism sprang. It is also the very stuff and methodology of magic, and, as Halliday suggests, for the very reason that personality is so widely and vaguely conceived. The magician works by his *mana* or *orenda,* that is, by 'contagion of qualities'; union or contact with power becomes in this way the foundation of both magic and religion; for "the wide area of personality as it is conceived in the lower culture, enables persons quite easily to be united or brought into contact with power."[58] Frazer's great collection of facts clustering about the tree-spirit, the king-priest-god-'vegetation-spirit,' who is killed annually, illustrates how this primal notion of the sensitive community between men, animals and cosmic powers animated both oriental and classical religion and myth.[59] It may also be said to survive in Christian ritual and tradition. The 'pathetic fallacy' of forcing Nature's moods into accord with those of suffering gods and heroes is as old as literature and still remains a trump card for the melodramatist or the romantic novelist.

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[54] For example, the Yakuts think fire is a *fussy old man*, always whispering and shuffling—a personality to be coddled. Recall that as late as the 13th century Pope Calixtus excommunicated a comet; and that frequently in the middle ages grasshoppers and other animals were gravely interdicted in courts of justice!

[55] "The idea seems adequately expressed by our term 'wonderful.'" (Boas, article 'Religion' in *Handbook of American Indians.*) But Hewitt says: "Those who interpret these terms as denotive simply of what is expressed by the English words 'mystery,' 'immortal,' 'magic,' 'sorcery,' or 'wonderful,' fail to appreciate the true nature and functions of the assumed power denoted by these terms as conceived by the Indians." (*Handbook of Amer. Ind.*, article 'orenda.')

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[56] See, e. g. Dorsey, 11*th Bur. Ethnol.*, 494, 467, 432-3, 365; McGee, 15*th Bur. Ethnol.*, 182; Miss Fletcher, xxvii, *JAI.*, 437; *id. Proc. Amer. Soc. Adv. Sci.*, 1897, 326; Spencer & Gillen, *N. T. C. A.*, 548, note; *id.* Northern Tribes, 629; Holles, The Masai, xix; Codrington, Melanesians, 118-9; Cambridge Exp. to Torres Straits, vi, 244-5; Hubert & Mauss, *L'Année Sociologique,* vii, 101 ff, 108 ff, 118, etc.; Hartland, *Man,* Mar. 12, 1912, p. 45; Lovejoy, *Monist,* xvi, 357-82; Hetherwick, xxxii, *JAI.*, 93-5; Jones, 'The Algonkin Manitou,' *Jour. Amer. Folk-Lore,* 1905, 183-91.

[57] For example, 'Virtue had gone out of him,' (Mark, v, 30; Luke, vi, 19, viii, 46, etc.); cf. also the Virtue in relics of saints and heroes. The visitor to the church of St. Etienne du Mont in Paris during the feast of Ste. Genevieve will see hundreds of persons still practising the doctrine of *mana* at the saint's shrine. A handkerchief or rosary becomes magnetized, as it were, by exposure to the saint's body or by touching her tomb.

[58] *Folk-lore,* xx, 147-67.

[59] Dr. K. Th. Preuss at the International Cong. for History of Religions at Leiden 1912, supported with great force this primitive notion of a 'magical unity.' The nearest relations as members of a group, he thinks, were regarded as a magic unity, corresponding to the tendency to regard groups of external objects such as the starry heavens, clouds, various forms of fire, an animal or plant species as a magic unity—of which at the same time each specimen represents that unity. In the light of what we know now of magic this seems clear enough. But we fail to see quite so clearly as Dr. Preuss seems to, how exogamy is derived from it by a natural process.

The third primitive law or principle, viz., that all changes, is simply a corollary to the one we have just considered. For if all is fused and undifferentiated in nature and there are no metes or bounds to the powers of men, animals, stones, and other members of this cosmic democracy, then it requires no stretching of the mind to conceive stock doing duty as stone, man becoming animal, or animal and stone giving birth to man. All barriers are down and the self so fluid that *this* can become *that* and *the other* at will. Abundant evidence is at hand to prove that this view of primitive sense of self is not fantastic nor exaggerated. We have already said that savage mind is rather zoomorphic than anthropomorphic. This is clearly brought out by von den Steinen:

> "To understand the way of thinking of the Indians we must throw aside entirely our notions of the division between man and other animals. A medicine man understands all the languages of beasts, birds and fishes. They have no notion of any ethical humanity. Being good and bad is only doing pleasant or unpleasant things to others. Abstract notions of good and ill, uncontrolled by fear or reward are wanting. The animals differ only in having no tools. Hence men arise from arrows and women from maize pestles. The Indian does not say "I am a man, and I think of the animals as acting like men." On the contrary men are thought of as animals, both in the good and bad sense. The tribes identify themselves and each other with animals. Some animals are also thought of as being the lord of particular plants, such as tobacco, cotton, mandioca, and other animals as lords of sleep or of the hammock, or of the jars filled with water. In fact, the Indian received the most important parts of his civilization from those persons whom he called animals. He owes to them the teeth, bones, claws, shells which he uses as tools and weapons. He is indebted for what he can do to the animals, his neighbors. Why should he not attribute to them the other things whose origin he does not know? The animals have not got those to-day because man has taken them away."[60]

The Tarahumares of Mexico, according to Lumholtz, feel that the animals are in no wise inferior to man, that they understand magic, know many things and can aid the Tarahumares in making rain. Similarly it is stated that a Bushman questioned by a missionary "could not state any difference between a man and a brute—he did not know but a buffalo might shoot with bows and arrows as well as a man, if it had them." "When the Russians first landed on one of the Alaskan Islands the people took them for cuttle fish 'on account of the buttons on their clothes.'"[61] "Some of the Ainos believe themselves to be descended from a bear and call themselves after it."[62] Among the Aruntas in a certain totemic ceremony an individual figures apparently indiscriminately as the rat-man or man-rat.[63] The Trumai of Northern Brazil say that they *are* aquatic animals. The Bororo boast that they *are* red paroquets. This does not mean merely that after death they will be transformed into paroquets, nor that paroquets are Bororos metamorphosed. Steinen says expressly (even though he refused at first to believe it), "the Bororos give it out coolly that they *are actually* paroquets (arras) exactly as if a caterpillar should say it is a butterfly."[64] It is not a name they adopt, nor is it a fancied relationship; it is an essential identity. And the same mental process no doubt holds in most totemic systems. This identification of primitive men with stocks and stones, with their totemic plants and animals, must not be thought of, however, in physiologic or structural terms, but rather in spiritual terms; that is, spiritual in the primal vague, loose sense. The identification consists in the fact that both are emanations or manifestations of a common energy or spirit behind all things. It is indeed a sort of pantheistic communism. This explains why, for example, the Bororos can actually think themselves paroquets even while they have no feathers and cannot fly.[65] It further explains the fact that such 'community' between men and animals does not stop there but may be extended to directions, localities and qualities; for example, to points of the compass, seasons, winds, colors. Hence probably the significance of orientation of graves, buildings, prayers; though such customs may also result from ancestor worship or the grave-cult. Hence, too, such 'sympathies' as the Baganda believe exist between a barren wife and the soil. A sterile Baganda woman may be repudiated because her sterility communicates itself also to her husband's garden.[66] Similarly in

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[60] Shingu Tribes (Berlin Museum, 1888), p. 351.

[61] Frazer, Golden Bough, ii, 108, note. Cf. Lubbock, Marriage, Totemism & Religion, 108-9, for sense of brotherhood between Peruvians and certain fish; also Hose & Macdougall, xxxi, *JAI*, for relations between men and animals in Sarawak. This zoomorphic

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tendency still lingers in modern thieves' slang. For example leather = skin; mufle or snout = face; bec or beak = mouth; wing = arm; flipper = hand. For a curious confirmation of zoomorphism from prehistoric archeology see Luquet, *L'Anthropologie*, xxi, 409-23.

[62] Lubbock, *op. cit.*, 109, citing Rev. J. Batchelor.

[63] Spencer and Gillen, *N. T. C. A.*, 231.

[64] Unter den Naturvölkern Zentral-Brasiliens, 305-6.

[65] Cf. Lévy-Bruhl, *op. cit.*, 100 ff; Hartland, Primitive Paternity, i, 191, 250. Jones in the article already referred to says that the Algonkins conceive of *manitou* as an objective presence, with the quality of self-dependence, without form or feature, "The most common experience seems to be that of being overwhelmed by an all-encompassing presence."

[66] Roscoe, xxxii, *JAI.*, 38, 56.

Japan only a young man is allowed to graft trees "because the graft needs all the vital energy possible." [67] A Welsh poem of the 12th century contained a sentiment to the effect that with false kings come failure of crops, bad years, and long days. Pokorny cites an example from the old Irish "Book of Leinster": under Cairbre Cinnchait who gained his throne by violence and caused the children of the nobles to be put to death without pity, each ear of corn bore only a single grain, each oak only one acorn. But when the old dynasty was restored Ireland regained her wonted fertility.[68] It is quite probable that many royal taboos (as Frazer and others have suggested) arose from this belief in sensitive relationship between man and nature.

To the set of ideas centering around this primitive communism between men and other creatures belongs the widespread custom of tracing tribal or family descent from plants, animals or inanimate objects. As we have already treated this subject elsewhere in considerable detail [69] it will be necessary here to cite only a few illustrations.

Bantu folk tales describe prehistoric times when human and lower animals were supposed to associate in marriage.[70] In the 59th Rune of the Kalevala (the story of the virgin Marietta), it is expressly stated that she conceived by a mountain berry:

> "Marietta, child of beauty,
> Thus became a bride impregnate,
> Wedded to the mountain berry," etc.

Later she gave birth to the child in a 'woodland manger' after a long series of painful adventures. In the same Rune the child is spoken of as follows:

> "Since the child is but an outcast,
> Born and cradled in a manger,
> Since the berry is his father," etc.

In China, a young widow claimed to have had a child *by the clay statue* of her dead husband.[71] The Indians of the Northwest claim descent from certain animals: "Francois is a member of that sept of the StsEelis whose remote ancestor was an otter. So I sought to learn from him whether they looked upon the otter as their relatives and paid regard to these animals by not killing or hunting them. He smiled at the question and shook his head, and later explained that although they believed their remote ancestor to have been an otter they did not think it was the same kind of otter as lived now. The otters from which they were descended were otter-people, not animals, who had the power to change from the form of men and women to those of the otter. All the animals in the old time were like that, they were not just common animals and nothing else; they were people as well, and could take the human or animal form at will by putting on or taking off the skin or other natural clothing of the animal."[72]

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[67] Chamberlain, Things Japanese, 449.

[68] *Mittheil. d. Anthrop. Ges. in Wien*, xxxviii, 34-45.

[69] Primitive Family as an Educational Agency, ch. iv; here will be found also illustrations of the converse phenomenon, viz., animals springing from human beings.

[70] R. H. Nassau, Where Animals Talk, 165.

[71] De Groot, Relig. Syst. of China, Bk. ii, (vol. iv), 342-5, cites this and other similar cases.

Now we come to still more specific examples of how nature's barriers are let down in primitive thought. Under the general term metamorphosis we shall include transformations of things into other things, things into persons, persons into things, persons into other persons.

A curious creation myth of the Mewan Indians illustrates the first phase of metamorphosis. After O-let-te and Wek-wek had created mankind, O-Let-te said to Wek-wek, "Now we also are going to change; I am going to be a hunting animal and you are going to be a hunting bird." . . . So O-Let-te the Coyote,-man, whose form up to this time we do not know, changed to the Coyote, a furry hunting animal. And Wek-wek changed to the Falcon, a hunting bird."[73] Even more striking is a myth from the Huichols of Mexico, according to which the wheat was at one time a deer.[74]

The old Greek legend of Pygmalion may be matched with many instances from modern ethnography to illustrate the second phase. In China statues of men and animals are believed to transmute themselves into real human beings.[75] And a similar belief has been observed among our North American Indians. Lafcadio Hearn recounts a Japanese legend of somewhat like tenor. "Also in those days there appeared in many provinces a Buddhist priest of giant stature, whom none remembered to have seen before and whose name no man knew, traveling through the land, and everywhere exhorting the people to pray before the bell of En-gaku-ji. And it was at last discovered that the giant pilgrim was the holy bell itself, transformed by supernatural power into the form of a priest. And after these things had happened, many prayed before the bell, and obtained their wishes."[76]

C. H. Harper, writing of totemism among certain Gold Coast tribes, says: "I have heard the story of a hunter who killed a deer and part of the body turned into a human being before it expired, but I believe it is a mere legend."[77]

European fairy lore with its tales of frog-princes, raven-princesses and the like furnishes parallel instances. In old German folklore the stork and human beings were commonly interchangeable by metamorphosis. New England witch-baiters were equally credulous. In the trial of the Indian woman Titiba, accused of witchcraft, it transpired that the devil appeared now as a tall man of Boston, now as a hog, now a great dog, a black dog, a man who threatened to hurt her; also apparently on occasions as a black or red rat. So far as the records show she was believed by her judges! The Egyptian belief that Ginnees (daemons) assume animal forms, notably cats and dogs, is another variant of this general belief. Ezekiel's vision of the valley of dry bones which became living men is perhaps an echo of it; and the dogma of literal transubstantiation as held by certain portions of Christendom is certainly a survival of it. Perhaps here should be reckoned also such crude 'miracles' as the liquefaction of St. Januarius' blood.

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[72] C. Hill Tout, xxxiv, *JAI.*, 335-6. This citation is particularly valuable because of Mr. Tout's qualifications and scrupulous care as an ethnographer.

[73] Merriam, The Dawn of the World, 87.

[74] Lumholtz, Unknown Mexico, ii, 45.

[75] De Groot, *op. cit.*, ii, 340-55.

[76] Glimpses of Unfamiliar Japan, i, 68.

[77] xxxvi, *JAI.*, 186.

The myths of Proteus, of Apollo and Daphne, the legend of Circe and her pigs,[78] and the well-nigh universal belief in were-wolves typify the metamorphosis of persons into things. Lot's wife is a familiar case in point. A curious variant of the Protean legend occurs in the folklore of the Golds (eastern Siberia). Fuji changes herself into a needle, a worm, a drop of blood, a gadfly, a skunk, and so on almost *ad infinitum.*[79] Puck in Midsummer Night's Dream is one of the same brood. Shakespeare's catalogue of his threatened metamorphoses reads like a chapter in folklore. Ellis states that among the Yorubas, "a belief in metamorphosis is universal, and is not limited to a change to an animal form, since men and women are sometimes transformed into trees, shrubs, rocks, or natural features. The shrub *bujo,* whose fruit is used to stain the skin in imitation of tattoo marks, was a Yoruba belle of that name, who was metamorphosed. . . . The Iyewa lagoon is also said to have been a woman."[80]

Lane reports an Egyptian belief in the metamorphosis of alum into human form, which is used as a charm against the evil eye.[81] Amongst the ancient Scandinavian peoples the most powerful form of magic was the ceremony of the *Seid:* in the course of it a woman could sometimes, while lying on a platform in the center of the group, change herself into another shape, often that of an animal, go to other cities and see what was happening there. If she were injured or killed during this period her body showed the marks.[82] The Kafir, says Mr. Kidd, "makes many strange but picturesque mistakes in localizing the 'self' . . . he is at such an elementary stage of thought that he imagines he can by magical charms, change himself into a wild animal and devour human beings, and then by magic transform himself back again into his original human shape."[83] The Malays have a tale of how a noted magician (a fishing-wizard) dived into the sea and became the porpoise; also of a child who fell into the river and became a crocodile.[84] Among the Serbs, at least until recently, it was commonly believed that people could be transformed into trees and plants, and conversely that human souls might be extracted from plants.

We are not concerned with the origins of the belief in wer-beasts, but only with that aspect of it which indicates a vague definition of the "self;"[85] hence a few typical cases will suffice. Among certain Gold Coast tribes members of the leopard family in particular believe that if the funeral custom is not well performed the deceased turns into a leopard and destroys the farms. Whenever a member of the family eats a plant called *susna* he turns into a leopard. Again, if the family do anything in violation of the dead man's wishes he will turn into a leopard and plague them; "the founders of the different tribes believed that a man could change his form after he is dead and take that of his sacred animal."[86] This case involves a particular sort of metamorphosis, viz., metempsychosis, complicated with totemism. Somewhat more characteristic is the Yoruba hyena superstition. A man may assume that disguise at night to prey upon sheep and cattle and, if opportunity offers, upon human beings. "Such man-hyenas are believed to be able, by means of certain howls and cries to compel people to go out to them in the dark forest to be devoured. A similar belief is found in Abyssinia."[87] Among the Nagas of Eastern Assam there is a belief in tiger-men. "These tiger-men, who are in league with the demons, are also fortune-tellers, and are much feared. They have the power of changing themselves into tigers and in this manner revenge themselves upon an enemy by killing him and his pigs and his cows. When they wish to change back into men, the tiger-man's wife must throw her clothes over the tiger's head, and at once he will change back into his human form."[88] Recently among the Ekoi of Southern Nigeria a chief fell under suspicion of having, in the guise of a wer-leopard, killed several cows and goats. The town was about to rid itself of him summarily when a white man arrived and put a stop to the proceedings. Among the same people Chief Agbashan is a mighty elephant hunter believed to have the power of changing himself into an elephant.[89] The metamorphosis of twins into cats by night is a superstition reported from southern Egypt.[90] So firmly is the wer-beast fixed in folk-mind that it expresses itself in children's play. Thus a favorite game of boys in the Malay Peninsula is the *Hantu musang* (civet-cat demon), in which a boy is "hypnotized" and "turned temporarily into such a beast by possessing him with the 'hantu of the musangs.'" If kept so for an hour there is danger of his becoming a real musang![91]

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[78] Cf. the parallel superstition cited by Lehmann: "Die thessalischen Weiber konnten durch Salben Menschen in Tiere oder Steine verwandeln." (Aberglaube und Zauberei, 50.)

[79] Laufer, *Amer. Anthropologist,* ii, n. s., 331-8.

[80] Yoruba-Speaking Peoples, 123.

[81] Manners & Customs of Modern Egyptians (Everyman's ed.), 256.

[82] Lehmann, *op. cit.,* 78.

[83] Savage Childhood, 66.

[84] Skeat, Malay Magic, 308-9, 283.

About the metamorphosis of persons into other persons clusters a variety of beliefs and customs involving witchcraft, daemonism, mortuary rites, initiation ceremonies, the 'changeling,' the 'fountain of youth,' etc. Perhaps of all these the renewal of youth is the simplest and most naïve. For example, in the Tauna Islands (New Hebrides) it is believed that in the olden times people could cast their skins and become young again.[92] Hence the Middle Age legends of the Wandering Jew, and of Joseph of Arimathea who subsists for centuries by the possession of that supreme relic, the Holy Grail, are not purely local Christian myths. The Yorubas have a belief corresponding somewhat to European changeling tales. In one version a nine months old infant grows up to a big boy and eats all the food the minute his mother is out of the house; and changes back to his infant form when anybody appears. The natives ascribe such a phenomenon to spirit possession. It is really referable to uncertainty about the 'self;' for, as we have already seen, the Yorubas also believe in wer-beasts and other phases of metamorphosis. Ellis refers to the "Father of Eighteen Elves" in Arnason's collection of Icelandic legends for similar stories.[93]

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[85] Yet in passing we might suggest that Lippert's theory of metamorphosis into were-wolves as a survival of ancient cannibalism is, to say the least, fine-spun, and a capital instance of bending facts to fit a general hypothesis.

[86] Harper, xxxvi, *JAI.*, 182.

[87] Ellis, Yoruba-Speaking Peoples, 122-3.

[88] Furness, xxxii, *JAI.*, 465.

[89] Talbot, *Nat. Geogr. Mag.*, xxiii, 34.

[90] A. M. Blackmar, "*Man*," x, 25-9.

[91] J. O'May, Playing the wer-beast, *Folk-Lore*, xxi, 371-4.

In all times one of the prescriptions of divinity or magic has been power to transform the 'self' of mortals. Thus Athene changed Ulysses into a beggar.[94] And one of the most hateful powers of Middle Age witchcraft was the ability to change a man into a maid, or *vice versa.* Divine power and magic could confer invisibility also by metamorphosis; but such metamorphoses were usually only a temporary device, unaccompanied by any real change in the person. Yet the fact that by donning a Tarnhelm one could see and not be seen still further illustrates primitive notions of the protean elasticity of the self. Initiation into the calling of magician or into a savage secret society was usually accompanied by a change in personality. Thus in West Africa the youth who is a candidate for initiation into the secret society is isolated and subjected to strict discipline, the whole purport of which is his death to the old life and resurrection to a larger life. He becomes a new man; even his name is changed; sometimes he learns a new language; he forgets or pretends to forget all his past life, even the commonest details of manners and customs; sometimes he must be taught even to walk and to eat.[95] In view of such qualities of mind as we saw in the

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[92] Gray, *Intern. Archiv. für Ethnograpie,* vii, 232.

[93] Yoruba-Speaking Peoples, 120-1.

[94] Odyssey, xvi, 194 ff.

[95] Similar customs in New South Wales, Queensland, Fiji, Congo Basin, New Guinea, Cerain, India, etc. See Lippert, Kulturgesch, ii, 341-2; Frazer, Golden Bough, ii, 342-57; *id.,* Totemism & Exogamy, i, 43-4; Chevrier, *L'Anthropologie,* xvii, 372-3; Kulischer, *Zts. f. Ethnologie,* xv, 194 ff; Bentley, Life on the Congo, 78 ff; Lawson, Hist. of North Carolina, 381.

Bororos, I am not sure we can call such customs *pretending* at all: the emotional coloring to belief may here easily beget a real experience of transformation, the birth of a new ' self.' Jones in the article already cited on the Algonkin Manitou shows clearly how just this emotional coloring *socially produced* determines the notion of the person in contact with that great vague but pervasive power in extension of the person which we have noted under the term manitou, mana, and the like.

Much more difficult of interpretation are cases of primitive impersonation. We are always at a loss to know how much is mere mimetic and how much genuine transformation into the part played. Take, for instance, a case from modern Servia. Prince Lazarovich-Hrebelianovich speaks of a curious invocation of the " Dodola," the mystic demi-goddess, personified by a gypsy girl, in whose keeping are the waters of springs and streams.[96] Or again a case from India:

> " On the death of a Dewan or of a priest a curious sport is customary at the funeral. The corpse is conveyed to the place of cremation on a car; to this car ropes are attached, and the persons attending the ceremony are divided into two equal bodies and set to work to pull in opposite directions. One side represents the good spirits; the other the powers of evil. The contest is so arranged that the former are victorious. Sometimes, however, the young men representing the demons are inclined to pull too vigorously, but a stick generally quells this unseemly ardor in the cause of evil."[97]

Here in this crude sort of morality play there seems to be a survival of old notions of interchange of personality between men and good or evil powers. The obvious display of sympathetic magic may easily reach back to more primitive notions of contact with that vague impersonal energy or *mana*.

From modern Russian ethnography comes a description of a Mordva burial custom which even more clearly illustrates the realistic interchange of personality:

> "The funerals wind up with a feast, songs, and sometimes dance. At harvest they leave on the field some bits of unharvested grain for the dead. There is a memorial of the dead on the 40th day especially, and frequently later. On the night before the 40th day, they clean up the house, repeating, " The father is coming, he will scold us." Early in the morning a person of the same age as the deceased goes to the cemetery; the others go to the courtyard where the ox destined for the memorial awaits his destiny; they make obeisance to

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[96] The Servian People, i, 75-6. The Dodola, he says is " certainly a survival from remote pagan times, when the children of men were the children of nature and felt themselves in very near communion with the trees and hills and the forces of sun, wind, and water."

[97] Lewin, Wild Races of S. E. India, 185.

> him to the waist; the women cover his horns with a cloth or bunch of grass; then they begin to lament, bowing to the ground before the ox. The ox led to slaughter represents the deceased. He is slaughtered by the nearest relative of the deceased, and cooked in a kettle with clear water. . . . After slaughtering the ox, they go into the cottage, where some prepare the meal for the expected guest, and others spread out on the lavka the clothing left behind by the deceased, in such a way as to represent a human figure . . . [then follow details of the feast preparations] . . . having prepared all that and lamented over the representation of the deceased, the nearest relative gives to all a cup of vodka or beer and invites them to go for the deceased. The guests make a deep obeisance, take the clothing, and with lamentation, go out into the procession. When they reach the grave they all fall down upon it, kiss the ground, and call the deceased. Then the Mordvine who had been sent out thither early in the morning speedily presents himself amongst the weeping relatives; the whole company flock about him with shouting, put upon him the clothing which they have brought, and taking him by the hand lead him back to the village. There they give him the lighted candle; they open the doors of the cottage; all make obeisance to the threshold and lead the pretended deceased with respect into the cottage, where they seat him in the front corner; the master or mistress of the house gives him a cup of vodka, or beer, and while he drinks all kneel before him on one knee, bending down their heads and leaning on the floor with one hand; then the master of the house entertains the guest. During this the guests go in turn to the pretended deceased, bow to him with reverence, and ask him about life in the other world. He offers each a glass of beer and gives them all reports about their relatives in the other world; thus: Yours has grown poor by spending; Yours keeps bees; Yours has become a drunkard; Yours has gone on a journey for his health. After the feast, they light a candle, around which the women stand and begin to weep for the guest, wishing that he may have light; then the company escort the guest to his own place. When they come to the cemetery, they take the clothing off him; thereupon he loses his fictitious character and sits down with the rest around the food which has been brought."[98]

It is said that the origins of Greek drama are to be found in ancient mimetic tomb-rites. They were probably of just the Mordva or even cruder type at first, and gradually, through aid of masks and other fictive means, attained independence and established themselves as a conventionalized art. Indeed probably all dramatic art rests on just such a realistic base, and gains, or at least gained its power from the *real transformations* ascribed to the actors' personalities. In the tomb ceremonies the living are conceived as investing themselves with the personality of the dead. But in the case of the Servians the person represented was conceived as living. Hence, in view of what we have already seen of primitive thought, it is not difficult to conceive of 'mimetic identification' between the living and either the living or dead. Of course it is difficult to determine just how much is realism and how much only make-believe; but we may venture to say that at some time or other concrete realism predominated. It is significant that even in Plato's day philosophers could fear the metamorphosis of the actor into his rôle.[99]

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[98] Russian Ethnography, 274 (transl. by W. G. Sumner in Mss. note); cf. Abercromby, Pre- and Proto-historic Finns, i, 177-8, quoting Smirnov.

It may not be out of place here to recall to mind such modern survivals of belief in metamorphosis as the change in personality of the priest when he dons his ecclesiastical vestments, the judge when he puts on his robe and mounts the bench, the convert when he claims "entire sanctification," the dogma of papal infallibility resulting from a mysterious interchange of personality between the pope and the Godhead; or finally the policeman when he lays aside his humanity and his citizenship to become the 'personification of the law,' and tells you with shocking naïvete that he tortures a suspected prisoner with the 'third degree' not as a man but as an officer!

IV. Widely scattered and unpromising as the materials may have seemed at first sight, they have yielded several important suggestions on the historical side of "human nature." In the first place we must have been impressed by the large rôle of the feelings in coloring primitive perception and especially the perception of the "self." This emotional coloring taken together with ignorance of the scientific order of nature expresses itself in a vague mystic outlook on nature in relation to the human self not very different from that expounded by Emerson in his Essay on Beauty.[100] The lack of sharp dualisms in primitive thought, in spite of the apparent absurdities to which it leads, may turn out to be not so absurd after all; especially the failure to distinguish rigidly between *ego* and *alter*. As an aspect of this primitive monistic thinking we must not fail to note its subordination of the individual self to that of the group. This we found reflected in primitive communism and in those broad definitions of kinship which indicate the merging of the individual into the totem-clan, Cult-Bund, or other group; and as a resultant, closely knit group solidarity. It appears, too, that the notion of the individual soul and its priceless worth came only long after the general sense of the 'self' as a member of human and cosmic society. Furthermore, we seem justified in concluding that the militant sense of self-as-property is a characteristic lacking in primitive men; that it is a characteristic acquired in the course of comparatively recent industrial evolution; hence, that it is modifiable. As to the possibility of modifying the human self, the widespread belief in metamorphosis seems to yield emphatic affirmation and is too deep-seated to be utterly repudiated. The phenomena of religious 'conversion,' of 'double personality,' of hypnotic suggestion, or of even more normal and commonplace educational experiences indicate that it may still retain a valid place in our thinking. Could we once peer into the depths of that dim valley, the subconscious self, we might well be startled at the undreamed-of possibilities of transformation and metamorphosis. But the final and most important conclusion from the ethnographic data we have gathered is that the sense of self is essentially social and that as the mind is a working unity, so the concept of self reflects this totality of mind—feelings, ideas, desires, percepts, concepts; and is controlled, shaped, and colored by it.

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[99] E. g. Plato, Republic, Bk. iii (Jowett): "the same person will hardly be able to play a serious part in life, and at the same time to be an imitator and imitate many other parts as well . . . human nature, Adeimantus, appears to have been coined into yet smaller pieces, and to be as incapable of imitating many things well, as of performing well the actions of which the imitations are copies."

[100] "Men hold themselves cheap and vile: and yet a man is a fagot of thunderbolts. All the elements pour through his system: he is the flood of the flood and fire of the fire; he feels the antipodes and the pole as drops of his blood: they are the extension of his personality."

A MARKED CASE OF DOUBLE INVERSION[1]

By George F. Arps, Ohio State University

I. Statement of the case and its family history
II. The subject's school record
    With reference to letters and words
    With reference to digits
III. The subject's response to unfamiliar words
IV. Tests in visual perception and tactual arrangement
    With cardboard letters
    With geometrical figures
    With one and two place numbers
V. Tests with the symbols viewed through the pseudoscope
    When symbols were presented in normal space order
    When symbols were presented in inverse space order
VI. Tests with familiar objects viewed through the pseudoscope
    Knife and chalk presented in different space relation
    Pencil presented in horizontal plane with rubber tip, first to right and then to the left
    Pencil presented in the vertical plane with rubber tip, first to the top and then to the bottom
VII. Theoretical considerations

I. Statement of the Case and Its Family History

The case here described is that of a small boy reported to the department of psychology by a teacher in one of the villages of Ohio. James entered the first primary and, at the time of writing, was seven years old. His ancestry is questionable, especially on the maternal side as the accompanying chart indicates.[2] The father, though ignorant and dull, bears a fair reputation for honesty and sobriety. The mother is illiterate, profane, intemperate, and was, prior to her marriage immoral as the family history shows. The field worker estimates the mother's mental age at seven or eight years while her boy, according to the Binet scale, tests out five and one-half years. Reference to the chart shows that James' grandparents on the maternal side were feeble-minded; that the grandmother on the maternal side was feeble-minded; that his mother and father are probably feeble-minded; that of two living sibs one is feeble-minded and the other at three years of age does not talk.

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[1] A paper read before the Ohio College Association at Columbus, Ohio, April, 1915.

[2] The chart and family history were prepared by Mrs. W. J. Koster, of the Bureau of Juvenile Research, Columbus, Ohio.

JAMES V.

III 5. James V., born Sept., 1907, in Ohio. Started to school in Sept., 1913; when he began writing, he wrote upside down and backwards. Child is small for his age, slender. Has clear brown eyes. Mother says he is susceptible to colds. Seems fairly bright considering surroundings. James is the oldest of his fraternity. The sibs are:

III 6. Flossie V., born 1909. Has a marked speech defect. Not averse to "showing off." Copies letters right side up, but is left handed. Has not yet gone to school. Seems of less than normal mentality. (Perhaps of the intelligence of a 4-year child.) Has brown eyes and hair. Small for her age.

III 7-8. Frank V. and twin, born 1911. Frank's twin died of pneumonia when small. Frank is over three years of age, and makes no attempt to talk. Clear brown eyes. Is plump, but mother says he is sick a great deal.

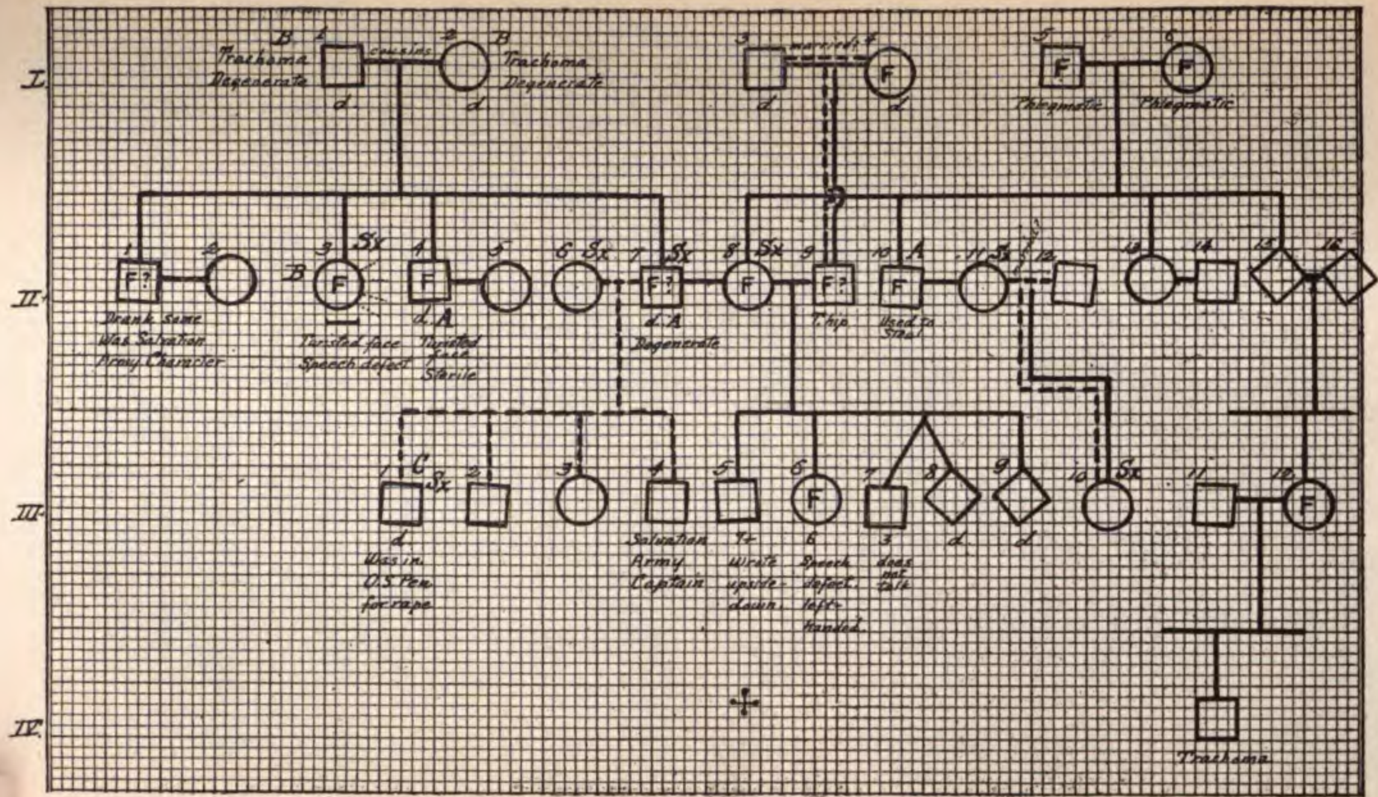
III 9. A baby born April, 1914, died Aug., 1914.

Father of III 5:

II 9. James V., Sr.—A small thin man, lame from tubercular hip; black hair, blue-grey eyes. Is ignorant and opinionated; doubtful if he is of normal mentality. Said to be a hard worker; doesn't drink, is not immoral. "Well-meaning." Chewed tobacco as he talked. Was disposed to be irritated because of the attention given his boy's case. It seems that the boy was kept out of school, as a trachoma suspect at the same time this mirror-writing tendency was discovered; and the father is not able to separate the two facts. Talked to me during my whole stay, insisting that nothing was wrong with the boy's eyes, and that no child could write correctly who had only been in school nine weeks. "They make them write letters before they know their A-B-C's now." I could not make him see why the case had attracted attention,—he was persistently obtuse (really or intentionally), as far as this was concerned. Railed querulously at Dr. Pomeroy and the State Inspector, for having suggested that anything was wrong with the child's eyes.

Lives in a poor, dingy neighborhood near the river. II 9 is the son of I 4, ——— V. She was a hardworking woman, not of high mental calibre. She was spare, bent and worn when informant knew her. Did washing to support herself and son. She claimed to be a widow, but it was not known that she was ever married.

Mother of III 5:


I
II
III
IV
B 1 Trachoma Degenerate d.
cousins
2 B Trachoma Degenerate d
3 d
married 4 F d
5 F Phlegmatic
6 F Phlegmatic
1 F? Drank some Was Salvation Army Character
2
B 3 Sx F Twisted face Speech defect
4 F d. A Twisted face Sterile
5
6 Sx
7 Sx F? d. A Degenerate
8 Sx F
9 F? T.hip
10 A F Used to Steal
11 Sx
12
13
14
15
16
1 C Sx d. Was in U.S. Pen for rape
2
3
4 Salvation Army Captain
5 7+ Write upside-down.
6 F 6 Speech defect. left-handed.
7 3 does not talk
8 d.
9 d
10 Sx
11
12 F
Trachoma

II 8. Mrs. Lucy V., age about 42. Lives in same town as James V. Poor neighborhood. House dirty and in some disorder. Since the flood, 1913, the family has been living in two rooms, which were replastered.

Mrs. V. is an untidy, frowsy woman, of low mentality (7 or 8 yrs.). She is rather stout, with pendulous abdomen. Brown hair and eyes. Chews tobacco as she talks. Seems amiable enough. Formerly immoral; never had venereal trouble. "Too dirty to catch anything," my informant said. During the 1913 flood, she had staying with her, a niece, III 12 ——— ———, with her family. This niece was feeble-minded. Was, if anything, dirtier than Mrs. V (II 8),—was positively filthy. This niece had a son, Chas. ——— (IV), who had papillar conjunctiva, and was taken to Columbus by the Blind Commission to be treated for trachoma. It was on account of close association with this boy, Charles, that James V. (III 5) was kept home as a trachoma suspect.

II 7. Lucy H. (II 8) was twice married. Her first husband was II 7, Si W. II 7 was an immoral degenerate (doubtless feeble-minded), who finally died of acute alcoholism. This Si W. was the son of I 1 and I 2, cousins, both degenerates, and both finally blind as a result of trachoma. These parents themselves are said to have been the result of inbreeding. Both are now dead. Si W. had three sibs of whom we know something. These are:

II 1. Alvin W. ("Chickie"). Lived near James V. Was a Salvation Army character. Drank some. (Probably of less than normal mentality.) Was married, but had no children. (Probably sterile.)

II 3. Jane W. Lived near James V. Now in County Infirmary. She is feeble-minded ("Never had any sense"), and before being taken to the Infirmary, was "common property." There is a suspicion of sterility here, as in spite of her sex activity, she never became pregnant. She has a marked speech defect. Her face is much twisted (asymetrical). She has conical cornea, and is nearly blind.

II 4. Dave W. Lived near James V. He was not considered bright. Face badly twisted ("could talk into his left ear"). Was a hard drinker and finally died of acute alcoholism. Was married, but had no children. (Probably sterile.)

Si W. (II 7), before his marriage to Lucy H. (II 8), ran with a woman named ——— S. (II 6), and she had four illegitimate children, presumably by him. These children were:

III 1. Clyde S. (illegitimate son of ——— S. and Si W.). Was in penitentiary for attempted rape. This was 25 or 30 years ago. The girl who made the charge was an immoral person; many think Clyde should not have been sent. Clyde died unmarried.

III 2. Walt S. Lives in a small Ohio town.

III 3. Stella S. Lives in same town as Walt S.

III 4. Jennison S. Lives in same town as Stella S.; is a Salvation Army Captain. A pretty decent man. Speaks on street corners.

Si W., II 7, later married II 8, Lucy H., but they could have no children. After Si's death, II 8 married II 9, James V., and they have five children, III 5, 6, 7, 8, 9, already described.

Lucy H., II 8, is the daughter of I 5 and I 6, ——— and ——— H. They were heavy set, phlegmatic, German people of less than normal intelligence. Lived on a farm. Lucy has three sibs of whom we know something. These are:

II 10. Sam H., a feeble-minded alcoholic, who in his younger days was also a thief. He married II 11, Kate K., an immoral woman, with an immoral daughter, probably illegitimate. Kate is brighter than her husband.

II 13. Jane H., married II 14, "Bluebeard A.," an old man many years her senior. They lived near James V., but have moved away.

II 15. ——— H., parent of the woman whose son was sent to Columbus for trachoma treatment.

This chart represents parts of three families,—The W.'s, V.'s and H.'s,—all of which are hardly up to the normal in intelligence. They are, for the most part, fairly industrious and inoffensive, and have given comparatively little actual trouble.

II. The Subject's School Record

During the first month of school James seemed hopeless; the only response made for this entire period, after many solicitations, was a statement of his name. He attempted neither writing nor reading and when threatened grew nervous and convulsive. In response to verbal symbols he seemed to be confronted with situations entirely foreign to his previous experiences. He usually withdrew from other children during the period of play except for an occasional game with smaller children whom he often bullied and abused.

At the end of the fifth or sixth week of school the teacher won his confidence sufficiently to induce him to attempt a copy which she had written on the blackboard. It was then discovered that the child invariably perceived the letters of the copy upside down and backwards. From this time on until Christmas all his writing, whether digits or letters, was inverted with respect to the up-down and left-right space relations, except in the case of the word 'cow' which he had painfully learned to trace. In three more weeks the words 'come' and 'me' were added, but very imperfectly. The constant tendency to inversion in the case of these words persisted as is shown by the frequent reversion to incorrect writing.

Unfortunately the earliest writings of the child were destroyed. The following specimens of his somewhat later writings are typical of the character of the writing of this period:

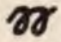
The words 'cow' and 'come' were copied in this manner—ʍoɔ, ǝɯoɔ.

In regard to her earlier efforts, the teacher has the following to say: "For days and weeks I tried to get James to write the word 'cow.' I had him go to the board and trace and retrace the copy with his fingers, then with chalk over and over again. Then he was asked to write the word with chalk or pencil as he had traced it; but invariably it was written in inverted order. One afternoon he succeeded, but the next morning all trace of the former success had disappeared." This oscillation of success and failure with reference to the practice word (cow) continued with decreasing number of failures until the holiday recess when he was fairly certain to write the practice word correctly. At the close of his sixth month of school James had learned to write with a fair degree of certainty the drill words 'cow,' 'come,' 'me' and 'James' with the letters in the usual up-down and left-right space relations.

The inversions peculiar to letters apply equally to eight of the nine digits. These were written as follows: ƨ, Ɛ, ᔭ, ϛ, 9, ㄥ, 8, 6.

The above account indicates a slow reconstruction of the left-right and the up-down directions. Unfortunately a complete history of the learning of the drill words is not available for the reason that the case was not immediately reported upon discovery. From the available material it is clearly evident that the reconstruction of the inverted directions does not take place simultaneously. Of the two inversions the left right is the more persistent, is less amenable to corrective experience. The reverse might be expected since the horizontal movements of the eye and arm are more readily made. It would seem, therefore, that the similar movements would be more readily coordinated into a system of space relations than the up-down movements of the eye and the forward and backward movements of the arm. But this is not the case. The earliest evidence of corrective experience is found in the up-down relation. Occasionally the digits 6, 7, and 9 are written as follows: ∂, Γ, Q The word 'cow' after weeks of effort is frequently written, 'woc.' In these cases it is obvious that the error for the up-down position has disappeared. But this correction is not permanent; the curious phenomenon of oscillation, mentioned above, recurs in that certain writings of the same words or digits are free from this error while others are not. At the end of the seventh month of school a few of the writings of the drill words and the digits still show both kinds of inversions; more of the writings show only the up-down inversion; while a very small number of writings are spacially correct.

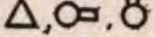
III. THE SUBJECT'S RESPONSE TO UNFAMILIAR WORDS.

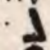
With reference to the practice words, once they were mastered, James responded with the usual promptness and confidence characteristic of a seven-year-old child. With reference to new words as 'me,' 'go,' 'nest,' 'see' and 'not' it was very difficult to secure a response of any kind. When asked to copy the word 'nest,' which the teacher had written on the blackboard, he appeared non-plussed, confused. After much persuasion he wrote the letter 't' but refused to write further. The word 'see' was then written on the board and copied as follows—'  . The word 'not' was next written and copied as follows—'ton.' The teacher wrote other words on the board, such as 'bring' and 'buy,' but no amount of coaxing or solicitation could induce him to undertake a copy.

The words 'see,' 'not' and 'go' contain literal elements not entirely new; such letters as 'e,' 'o' and 'n' were met in the practice words, 'cow,' 'come' and 'in.' Hence we find him making attempts to copy these words. But in the words 'bring' and 'buy' the right-hand letters, 'g' and 'y,' with which the writing reaction in the case of James must begin, are strange and unfamiliar. Moreover, the letters 'g' and 'y' are intrinsically difficult.

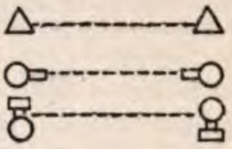
IV. TESTS IN VISUAL PERCEPTION AND TACTUAL ARRANGEMENT

The purpose of these tests was to determine to what extent tactual and visual perception of directions cooperated in the reconstruction of the subject's up-down and left-right relations of individual letters, of letters in words, of individual digits and digits grouped in two and three place numbers. Geometrical figures were also employed. To this end the teacher cut out the letters of the alphabet and the nine digits from stiff cardboard paper. The various geometrical figures were cut out of sand paper.

The cardboard letters, 'c,' 'o,' 'a,' 'w' and 't' were now placed successively in the subject's hands carefully screened from view. Each letter was twice carefully identified to make sure that it was accurately apprehended. The word 'cow' was then written on the blackboard and James was asked to spell and pronounce it. Immediately after this he was asked to arrange with his fingers the letters, 'w,' 'o' and 'c,' which had been placed promiscuously in his hands, according to the order in which he perceived them on the blackboard. The letters were carefully screened from view while he arranged them on a table before him. A like procedure applied to the words, 'come' and 'cat;' to the digits, 7, 6, and 4; to the geometrical figures,  ; to the letters, 'm,' 'w,' 'y,' 'a;' and to the two and three place numbers, 45, 69, 37, and 456, 679, and 283. The results are as follows:

Visual Copy	Tactual Arrangement
come	we
cow	wo
cat	ta
M	W
W	M
Y	⅄
A	Ɐ
7	
6	
4	
45	ςϸ
69	69
37	ƐL
456	459
679	6*6
283	Ɛ8Ƨ

Visual Copy	Tactual Arrangement



The experiment was then repeated twice for each of the words, 'come' and 'cow,' with the following results:

Visual Copy	Tactual Arrangement
come	omɘ, ow
cow	mo, wc

V. TESTS WITH SYMBOLS VIEWED THROUGH THE PSEUDOSCOPE

In these tests the pseudoscope was placed on a box in a position parallel to the line of sight and focused on a definite area of the blackboard. Within this area all letters, words, digits, two and three place figures, and geometrical forms were sure to fall within the visual field of the pseudoscope. The same symbols were employed as in part III with the addition of the word 'rat.' The method of presentation follows that of part III except that in this experiment the sym-

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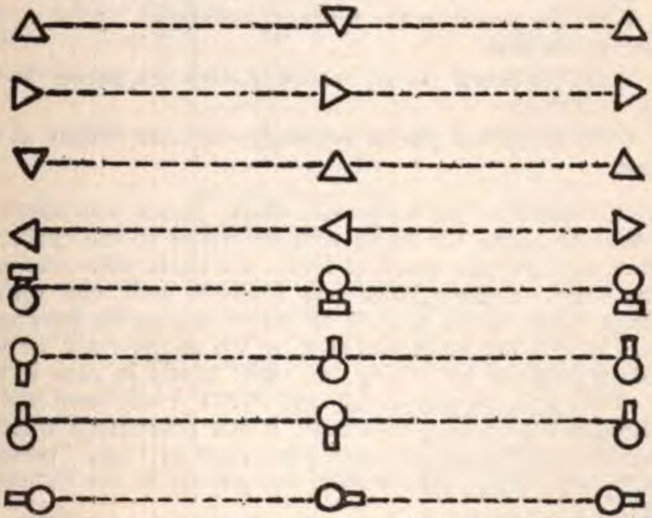
* The 7 in this group of numerals was reproduced in inverted and mirrored form, *i.e.*, the horizontal bar of the 7 was downward and it was turned to the left.

bols were exposed in two series. In the first series they were written or drawn on the focal area in their proper spacial relations. In the second series they were written or drawn in such a way as to invert the normal space relations, *i. e.*, they were presented upside down and backward. The results follow.

Symbols	*Seen through pseudoscope when presented in the normal space relation*	*Seen through pseudoscope when presented in inverse space relation*
cow	cow	ʍoɔ
come	come	ǝɯoɔ
rat	rta	ʇɐɹ
cat	cɐʇ	tca
7	7	ㄥ
6	9	9
4	4	ㄣ
M	M	W
Y	⅄	Y
A	A	∀
45	4ϛ	ϛㄣ
69	*9	6†
456	45*	9ϛㄣ
679	6ㄥ9	679
283	28Ɛ	Ɛ8Z



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* The six appeared in inverted and mirrored form, *i.e.*, the loop was upward and was turned to the right.

† The nine appeared in inverted and mirrored form, *i.e.*, the loop was downward and was turned to the left.

The results of the pseudoscopic tests confirm the observations made under normal conditions. Here as there the phenomenon of oscillation is evident; that a slow process of reconstruction of space relation in response to school demands is going on is also evident.

VI. Tests with Familiar Objects Viewed Through the Pseudoscope

In these tests three common objects were employed—a piece of common chalk shaved down on opposite sides, a pocket knife and a rubber capped pencil. The pseudoscope was again placed on the box and the objects brought into the field of vision. Five readings were taken of each of the following positions:

1. With the knife on top of the chalk.
2. With the chalk on top of the knife.
3. With the knife to the right of the chalk.
4. With the knife to the left of the chalk.
5. With the pencil lying on the table with its rubber tip to the right.
6. With the pencil in the reverse position of "5," *i. e.*, the rubber to the left.
7. With the pencil placed vertically with the rubber at the top.
8. With the pencil placed vertically with the rubber at the bottom.

With respect to the knife and chalk, James was asked at one time to locate the knife with reference to the chalk; at another time he was asked to locate the chalk with reference to the knife. Chance order was followed both with respect to the position of the objects or object and to the interrogations. In case the knife was seen on top of the chalk, he was asked to respond by raising his right hand; in case it was seen to the left, he was to respond with the left hand and so on through the series. Previously it was determined whether James clearly understood what was meant by 'top,' 'bottom,' 'right' and 'left.' The results are shown in the following table:

TABLE I

Position of Objects. .	+	—	—	—	+	—	+	+	—	+	R	L	L	R	L	R	L	L	R	R	r	r	P	r	P	P	r	P	r	P	T	B	B	B	T	B	T	T	T	B
Response	C	C	C	C	I	C	C	C	C	C	C	C	I	C	I	C	I	C	C	I	C	C	C	C	C	C	C	I	I	C	C	C	C	C	C	C	C	C	C	C

+ means knife on top of chalk
— means knife below the chalk
"R" means knife to right of chalk
"L" means knife to left of chalk
"r" means pencil was placed with the rubber tip to the right
"P" means pencil was placed with the rubber tip to the left
"T" means pencil was placed vertically with the rubber tip at the top
"B" means pencil was placed vertically with the rubber tip at the botttom
"C" means correct response
"I" means incorrect response

In six unrecorded cases of left-right confusion the subject failed entirely to respond.

Out of a total of forty responses only seven errors were made. Of these errors six inverted the right and left directions. It is probable that these errors are such as would likely occur even in the normal child of seven years of age. Significant, however, is the fact that all errors, with one exception, should be of the left-right order, since it was observed above that the inversion of the left-right direction is more persistent than the up-down direction. It is also significant that the six cases in which the subject failed entirely to respond should be cases involving the left-right direction. This is suggestive of the first stage in the reconstruction in the space relation of letters in words. With the elimination of the up-down error James' writing parallels that of the common cases of mirror-writing.

VI. Theoretical Considerations

The curious mixture of errors, the peculiar alternation of correct with incorrect spacial relations presents an interesting problem in the reconstruction of space perception though a new association of visual and tactual elements. Reference to the experimental data indicates an inversion of the vertical and horizontal visual space relations, but this does not appear to be the case when the same relations are explored tactually. It is possible that touch operates correctively to the original visual experience so that the inverse visual relations take on the normal meaning of top-bottom and left-right relations.

In the seeing person the tactual and visual elements constituting directions form an incomplete fusion so that the relation between the two groups of elements is more or less lax. For this reason a given fusion may, through a change of conditions, be undone and an entirely new fusion may be reconstructed. This may be illustrated by an attempt to trace a figure by means of a mirror in which case the systems of visual and tactual directions must be differently associated than has hitherto been the case in experience. The experiments of Stratton[8] illustrate the undoing of the associative bonds which experience has established between visual and tactual direction; they also illustrate the reconstruction of a new association.

In the case here reported it appears that the original associative bonds between the visual and tactual systems of direction were broken down and a new association established, whose sensory elements were the direct opposite of the original association.

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[8] *Psychological Review*, IV, 1897, 341-360 and 463-481.

What the teacher is apparently attempting here is to reharmonize disparate space relations through experience. In the normal individual there is probably more or less cooperation between visual and tactual space perception from the very beginning. Through experience this cooperation is made definite and effective so that the space relations gained through these two sense departments become fairly homogeneous. Under normal conditions the two sets of relations may easily be harmonized but in the case under discussion it may be assumed that the visual and tactual space relations were at first inversely related with respect to the normal order and for this reason the two sets of relations reharmonize with difficulty. Instead of correspondence and cooperation we find rivalry and antagonism —the verbal visual left-right and up-down directions contending with the corresponding tactual directions.

Under normal conditions the child just entering school has before him the enormously complex process of correlating the various sensations involved in forming accurate visual and tactual space relations common to words. The finer accessory movements of hand and eye involved in learning verbal symbols for the most part remain uncorrelated until the age of entering school. The underlying mental equivalents of tactual movements involving tactual local signature through experience take on the meaning of the mental equivalents underlying ocular movements involving visual local signature, or vice versa. The series of local signs meaning a given direction in one of these sense departments find a corresponding significance in the local signs of the other. The disparate sets of signs in the case here investigated appear in discord and the development of a fusion of disparate series of corresponding signs is delayed. The earliest responses of James seem to indicate that the meaning of a series of signs in one sense field stood opposed in meaning to the corresponding series in that of the other field. The opposition is clearly in evidence during the period of reconstruction under the direction of the teacher. The persistency of this opposition is evidenced by the " over and over again " tactual tracings of the initial, visually observed, words (' come ' and ' me ') and the oscillation of correct and incorrect writings. The reharmonization of the vertical and horizontal directions proceeded with unequal difficulty; the horizontal reconstruction is especially obstinate as is shown by its tardy disappearance.

The efforts of the teacher were thrown on the side of the tactual direction in an effort to bring about correspondence with the visual direction. The enormous number of tracings involved in learning the word 'cow' and the consequent indifferent success does not necessarily establish the primacy of vision for the vertical and horizontal position of digits, letters and the relations of letters in words. Since the tactual directions when tested independent of vision conform more nearly to that of the normal child's writing, progress in the case here described appears dependent upon the subordination of visual direction to tactual, *i. e.*, the visual space directions through the repetition of tactual experiences take on the meaning of the latter.

At first those reactions were eliminated which diverged most from the normal order; successive trials and errors is gradually bringing out of the confusion a fairly consistent and stable space relation of verbal elements and digits.

THE SIGNIFICANCE OF CLOTHES

By Sylvia H. Bliss

"The Horse I ride has his own whole fell: strip him of the girths and flaps and extraneous tags I have fastened round him, and the noble creature is his own sempster and weaver and spinner; nay his own bootmaker, jeweler, and man-milliner; he bounds free through the valleys, with a perennial rainproof court-suit on his body; . . . nay, the graces also have been considered, and frills and fringes, with gay variety of color, are not wanting. While I—good Heaven—have thatched myself over with the dead fleeces of sheep, the bark of vegetables, the entrails of worms, the hides of oxen or seals, the felt of furred beasts, and walk abroad a moving Ragscreen, overheaped with shreds and tatters raked from the Charnel-house of Nature, where they would have rotted, to rot on me more slowly."—*Sartor Resartus*, Thomas Carlyle.

Because of our confirmed habit of apparel the question, Why did we wear clothes? is seldom raised. Obviously as individuals, living in the twentieth century, we clothe ourselves because it is the custom; and further, to conserve the heat of the body and to satisfy the claims of modesty. Adding to these reasons the large share played by personal vanity the matter appears to have been settled out of court, leaving no cause for further prosecution of the case. But if we slip the noose of our habitual attitude toward the human race, regarding ourselves simply as members of the zoological genus Homo and related by ties of blood to all other living creatures, this unique and distinguishing habit of dress seems to require further consideration. Why does man thus add to nature?

Custom, the mightiest force operative to-day in the matter of dress, is invalid as a reason for the inauguration of clothing, custom being but the repetition of acts and observances already introduced. In view of the probable fact that the primordial home of man was in tropical or sub-tropical regions, and in the light of what we shall presently find to be the character of rudimentary clothing, the protection of the

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Note.—In addition to the authorities named in the text the writer is indebted to the following works: "The History of Mankind," Ratzel; "The Races of Men," J. Deniker; "Woman's Share in Primitive Culture," O. T. Mason; "Cyclopædia of Costume," Planche; various articles in cyclopædias and magazines; and to Robert H. Lowie, Associate Curator of the American Museum of Natural History, New York.

body from cold can hardly have served as the primary motive. Even in a climate so harsh and changeable as that of South Australia where there is apparent need of protection the natives are totally naked or wearing only a body ring. Coming to the consideration of modesty as a reason for clothing we find ourselves upon no firmer ground. The manifestations of modesty or of shame at the violation of "physical self-respect" are by no means confined to one portion of the body. In different nations the head, face, breast, foot, knee, or finger tips must be covered in deference to decency, while among peoples accustomed to tattoo or paint the body it is considered immodest to appear without these decorations, to a Carib woman the omission of paint being a more serious offence than the failure to don her girdle. On the other hand it is related of an aboriginal girl in the valley of the Orinoco, that having put on a gown to please a European visitor, she was much abashed when a member of her tribe appeared, and hastily threw off the garment. Shame would thus appear to result from violation of custom and close study of the facts has led several writers, notably Westermarck in "The History of Human Marriage," to the conclusion that ideas of modesty are purely relative and that shame is not the cause but the result of clothing.

That the initial impulse to dress came from the desire to render the body more attractive is a conclusion receiving support from the science of ethnography which has established the fact that in the evolution of clothing ornament preceded dress. One authority states that "the first and most primitive mode of personal adornment is certainly that in which the body itself is adorned without the putting on of any extraneous objects whatever." And of these adornments it is conceded that the daubing of the body with coloring matter obtained from colored earths and the juice of certain plants is the most primitive. Nearly all peoples who go naked thus adorn themselves. Of the Australians, for example, it is said, "Even the poorest and most wretched do not forget to paint their bodies," and the Fuegians daub faces, hair, and occasionally the entire body with pigments, red, yellow, white or black.

It has been asserted that the scars and blood stains displayed by the returned warrior, serving as a mark of distinction and honor and further as a sex lure, constituted the first decoration, giving rise at length to painting and tattooing. In regard to the latter practice this theory finds support in the custom of the Papuans of New Guinea with whom tattooing upon a man signifies that he has killed some one. From accidental to deliberate scarring of the body is but a short step.

The habit of ornamentation, if it may be thus designated, grew apace. Tattooing in all its varied forms, mutilation and deforming of the body, breaking and filing of teeth, objects inserted in the ears, nose and lips, elaborate arrangements of hair with ornaments attached, and at length strips of hide and sinew of animals and herbaceous twigs were fastened around parts of the body where there was a depressed surface, above a bony projection or a muscular protuberance—the neck, the waist, the wrists and ankles, " as is still seen among the Fuegians, Melanesians, Bushmen and Australians." Here we have the prototype of the girdle, collar, necklace, belt and bracelet. To these were attached secondary ornaments, shells, seeds, bones, feathers, fur and flowers, and finally a beast's skin to form a mantle hung from the neck and to the girdle were fastened leafy branches, pieces of bark, tiny aprons of human or animal hair, and coverings of feathers and grass, the rude beginnings of the skirt. A somewhat crucial matter and one begetting controversy is that of the origin of the fig leaf, loin cloth or apron. In the book of Genesis is probably the oldest attempt to account for clothing and there it is attributed to the sense of shame. " And the eyes of them both were opened and they knew that they were naked, and they sewed fig leaves together and made themselves aprons." Carlyle, with later knowledge and insight wrote: " Shame—as yet a stranger to the Anthropophagous bosom, arose there mysteriously under clothes; " and his conclusion is quite in line with the modern conviction that clothing gave birth to modesty. Several authors and especially Westermarck contend that the various coverings above enumerated are not for the purpose of concealment but to make prominent and draw attention to the parts of the body thus hidden, this being one of the most powerful means of sexual selection.

Some investigators find in the jealousy of husbands a reason for the origin of clothing while Havelock Ellis, in " The Evolution of Modesty," contends that it was the man who first covered portions of the body and this not from motives of modesty but for protection, his organism being the more sensitive and his activities of wider range. In view of the fact that loss of the tail and assumption of the upright position left the body peculiarly vulnerable this theory has much to commend it. A further view relating more especially to decoration, is championed by J. G. Frazer, in " The Golden Bough," and by Ernest Crawley in " The Mystic Rose," and is to the effect that mutilation and ornament exert a magical influence over the various organs of sense and serve to guard and insulate dangerous bodily functions, being practically a "permanent amulet or charm." Others have called attention to the superstitious factor in clothing, which attributes to small objects and trophies a beneficial influence, "tending to produce in the wearer the attributes of the object or of the whole of which the object was a part."

Discriminating and applicable though these theories may be it is obvious that no one of them adequately accounts for the fact of clothing nor sufficiently explains its complexity and variety. It may be, as one writer suggests, that the ancient Britons painted the body with earthy pigments to check the cooling effect of free evaporation from the skin; that the Andaman Islanders plaster themselves thickly with mud in order to resist the attacks of insects; the skin mantle of the Fuegian, shifted to meet the varying winds, and the elaborately fitted fur garments of the Eskimo, are obviously worn in deference to rigorous climate; the gourd or sling of certain South American tribes probably serves as a protection from injury, and the exceedingly small pearl-decorated apron of the Kafir belle is doubtless worn as a means of attraction; vanity, aesthetic feeling, the desire for distinction and the motive of comfort play their part. But as the primitive clothing impulse manifests itself in such varied forms we are justified in retreating beyond these partial hypotheses to one more profound and fundamental which underlies and includes them all.

Too great stress must not be laid on the factor of use, on an assumed end determining the particular form taken by the primitive impulse to decorate or clothe the body. The doctrine of use as a factor in evolution finds less favor than formerly. In the language of Professor William Patten of Dartmouth College, "The use made of an organ can not be the cause of its origin, for the organ must be present in the first place, in some form or other, before any use can be made of it:" and while to-day we find man by reason of his acquired equipment of reason and foresight working toward definitely conceived ends, it is hardly reasonable to attribute to the primitive creature at the outset of the human career clearly defined motives which determined his acts. As has been pointed out by the naturalists Geddes and Thompson, human nature can not be rightly understood apart from the biological approach, and even in a matter apparently so far removed from the natural as that of clothing there will be found many analogies to zoölogical and biological facts. Primitive psychological attitudes arise from what has been termed physiological thought and the instinctive inner urge prompting the acts of primitive man may be not inaptly compared to those special internal conditions which biologists recognize as determining local growths, organs and structure, lower down in the scale of life.

In order adequately to frame a philosophy of clothes it is necessary to view as clearly as possible man's place in nature. Though there are now on earth only isolated examples of hairy men it is probable that the primitive human being and certainly his precursor were covered with hair. We may or may not accept the Darwinian conclusion that the loss of our coat of hair was due to aesthetic reasons, " the members of one sex having chosen as mates those of the other who were least hairy," but the fact remains that man, as Carlyle said, is by nature a naked animal. Moreover he is, broadly speaking, the only naked animal. In the world of living things are displayed fur, feathers, thickened and colored hide, scales, various armors, and integuments, for the tree bark and for all plant forms fitness and beauty of investiture. Man alone is left with an incomplete exterior. His position in nature is anomalous. All other creatures are finished and complete, clothed and with instinct sufficient to form themselves an abode which remains unaltered with the passing of the ages. Man alone must supplement nature. He has progressed by reason of his incompleteness and to what extent his initial advance was due to the lack of a satisfactory and fitting exterior is matter for conjecture. The gods left man naked in order that he might clothe himself: unfinished that he might indefinitely continue the process of development.

Underlying all the various motives which apparently lead man to paint, tattoo, decorate and protect the body is the fundamental feeling of incompleteness, of dissatisfaction with self as it is, and clothing in its origin and subsequent development is the result of his attempt to remedy the deficiency, to replace what he has lost. The covering and ornament which human beings supply for the body stand in lieu of fur, feathers, and all the varied exteriors found in lower nature and further, serve like ends of protection and adornment. The fact of the reputed complete nakedness of certain peoples does not militate against this theory of the primary reason for clothing. While individuals may be entirely nude it is said that in no tribe do all the members remain constantly as nature left them. Study of " our contemporary ancestors " discloses, it is probable, most of the forms of adornment and body covering used by prehistoric man—complicated in many instances by contact of the savages with civilized races—and as might be expected there are peoples in whom the clothing impulse has not developed, or but feebly, going no farther than paint, the mutilation of some organ or the wearing of a necklace or belt.

Carlyle found the first spiritual want of a barbarous man to be decoration and while it is difficult to connect with any form of spirituality the all but incredible mutilations practiced by certain tribes, if we interpret spiritual want in terms of dissatisfaction with self we may indeed find in these primitive attempts to alter the body the germs of that discontent that is termed divine. The Bongo woman encouraging an exuberant growth of proud flesh to form embossed ridges on her arm, the Botocudos of Brazil with large flat discs of wood worn in slits cut in the ears and under lip, the Papuans and Australians perforating the nasal septum to hold a bone or stick, the Masai of East Africa with ear lobe enlarged to many times its natural size by a stretching stone, Chinese with compressed feet, Indians with flattened skull, are all impelled by motives deeper than those commonly imputed. Though the custom of deforming the body has all but died out in civilized races, clothing has until very recently strangely and often grotesquely disguised the natural form. During the fourteenth and fifteenth centuries it is said there was hardly a part of men's bodies that was not made to look deformed by their clothing.

Man's place in nature must be still further defined if we are to appreciate to the full the significance of clothing. Humanity appears to be a continuation of the main stem of life of which all lower forms are the branches. They diverged from the central stem, advanced a pace and became what Nietzsche would term the goals of nature, plant, insect, animal and bird, —man, according to the German philosopher's thought, being not a goal but a bridge. The life which was to become human continued to advance though divested of many possibilities. It is not necessary to accept all the implications of Bergson's philosophy in order to make use of his pregnant idea that life, evolving in the direction of man has abandoned many things by the way. Tendencies which were incompatible with the main trend of life were dropped and set up a subordinate line of development. Applying this conception to the matter in hand we may say that man has left far behind the possibility of a furred or feathered exterior, of blossoms, thorns, horns, tails, and countless other structures and appendages displayed by lower forms of life, plant and animal. It may be said further—and here is the crucial point in our philosophy of clothes—that these structures, appendages and ornaments which are characteristic of life other than human, survive in man as subconscious dispositions which at various times in the world's history, some in one race and some in another, are embodied in his dress. Actual physical survivals of lower forms of life appear during the development of the human fetus. Certain of these disappear, others are modified to form working parts of the organism, while occasionally one persists as an atrophied structure in the fully developed human being. In the light of these facts we are warranted in assuming the presence of corresponding mental survivals.

The variety and vagary of garb are thus not due to mere whim and vagary of the human mind. Man is the epitome of all tendencies and the reason for the complexity of his clothing impulse may be found in the complexity of his mental inheritance which includes all that he has lost physically on the way to man. There is scarcely a covering in nature that has not been utilized or imitated in human apparel; there can hardly be found a protuberance or appendage that may not have served as the prototype for some form of human mutilation or adornment. Fur serves both savage and civilized man. Certain tribes of the Amazon basin fix a covering of feathers on their bodies, daubed with a sticky substance; other tribes insert feathers in perforations in the cheek or nasal septum, while feathers as adornment, especially of the head are found the world over and not least in modern civilized nations. There are striking simulations of horns, notably the head-dress of some African tribes, and in England what has been called "the preposterous horned head-dress" of the reign of Henry V. The student of costume will come upon many an arresting likeness of coronet, cockade, neck ruff, stock, and frill, plume, sash, and train, to natural organic characteristics of other creatures and it is interesting to note in passing that a caricature of the date of 1786, entitled "Modern Elegance," shows two women wearing the Bouffon, an exaggerated neckerchief of cambric, and above them the figure of a Pouter pigeon with characteristically inflated œsophagus.

Perhaps the most striking example of physiological habit surviving in man as a mental tendency is that of the tail. This appendage has been so often simulated that it has given rise to the fable of men with tails and even our modern sash and train may, without stretch of the imagination, be referred to a like lowly origin. The student of savage costume comes again and again upon instances of this addition to man's natural equipment and while the claim may be made that this widespread habit is due to imitation of animals it may with greater reasonableness be attributed to the subconscious reminiscence of an actual tail. This view is strengthened by the fact that the tail-like ornament is often worn on the front of the body and quite naturally the conclusion is reached that the various forms of the fig leaf, apron and clout may be included in the same category. The tail being one of the most recent of our losses, physical vestiges of this appendage occasionally, it is said, persisting in man, the impulse to thus supplement the body is strong. Deeper than the ends which they serve is the reason for all forms of apparel.

This conclusion is applicable also when we come to consider the relation of clothing to the fact of sex. The doctrine of sexual selection—the development of beauty of coloring, ornament and the like through their influence in courtship and consequent increase through inheritance, has been subjected to much criticism since its promulgation by Darwin, but whatever theory finally prevails the fact remains that the attainment of sexual maturity and the arrival of the period of reproduction,—seasons which in all life but human are practically coincident,—are marked by all the beauty, elaboration, fragrance and exuberance of which the organism is capable. Secondary sexual characters such as manes, beards, crests, tusks and antlers, combs, wattles and top-knots, are acquired with the approach of sexual maturity and frequently are retained in their full glory only during the reproductive period. Of the deer it is said that the production of antlers is intimately connected with the generative function and in many birds there are developments peculiar to the breeding season, such as the great throat pouch of the male bustard and the egret's tuft of long and delicate feathers. The scent glands of many creatures are more active at this season, the beauty and fragrance of the flower—the flower itself—are signs of reproductive power, and the entire imago state of many insects,—that brief period of beauty that of all their life cycle is the only one known to most of us,—exists solely for the purpose of mating and depositing of eggs, the insects taking no food, the mouth parts of certain species being completely atrophied. With this heritage of tendencies it is but natural that man, the epitome of all lower life, should display a like close connection between the development of the sex function and exquisite attire and with his greatly lengthened period of courtship enormously increase the beauty and magnificence of his costume. Here again we are but carrying out laws which in nature are organic. Antedating and underlying the conscious motive of sex-appeal is the instinctive impulse to beautify and adorn the body.

That it is the female who now, among human beings, is distinguished by beauty and magnificence of apparel is a fact which controverts biological precedent, for in other forms of life where the sexes differ the excess of display is generally with the male. This condition appears to have obtained also in clothing up to recent times and gradually gave way to our present " dead level of negative commonplace and drab, gravelike nonentity " for the reason that man, as clothing more and more covered the body, found the accumulation of drapery a hindrance to his strenuous activities. With the Romans two costumes were adopted, one for labor and one for ceremonial occasions and finally the utilitarian style dominated men's attire, the decorative function being represented by women's dress. It is probable that the modern male mind is influenced likewise by the motive which led Montaigne to discard the polychrome clothing of his age. He declared that he would not be bothered about deciding the color of his garments and so wore only black and white. In numberless ways has the primitive clothing impulse been modified to meet the contingencies of civilization.

It is easy to attribute change of fashion to mere caprice or to mercenary contrivance on the part of dressmaker and milliner. Easy and natural also to decry as vain or dandified the individual whose efforts are concentrated on exquisite dress. This is but a superficial view of the matter. In all his efforts of this character man is guided not only by the impulse to rehabilitate himself with all that he has lost on the way to the human level but to attain as well the absolute freedom, comfort, suitability and beauty of attire displayed elsewhere in nature. Dominated by this unconscious ideal he is dissatisfied with all ill-fitting, unsuitable, unlovely garments and age after age strives for the perfect human costume, one which for man shall be as fitting, natural and characteristic as the exterior of fur and feathers for animal and bird. But he rarely more than approximates this condition. Confronted with the difficulty of making his garb conform and give expression to an ever growing spirit he follows a flying goal. Setting aside the servile imitation which is the motive moving multitudes in their choice of garments we find beneath every important change of style a change of mental outlook. Our oft-derided fashion makers may be more closely in touch with the spirit of the age than we dream, registering in their creations profound movements of the human soul.

The fact is significant that, generally speaking, in the East costume has undergone relatively little change as compared to the West with its mingling of peoples and where civilization is complex and unstable. The most rapid alterations of style attend on swift changes of ideas, ideals and conditions. In France where civilization has attained a very high point dress reaches its finest, most exquisite development. Countries isolated and homogeneous long preserve the distinguishing national costume while with the breaking down of individuality by mingling with other races the dress becomes heterogeneous and complex as at the fall of the Roman Empire and in the Japan of to-day.

In consequence of the instinctive desire for absolute fitness of apparel we find human beings inventing distinctive costume for every condition and occasion,—youth, age, rank, occupation, war, worship, the funeral and the dance. This custom has its beginning among peoples low in the scale of civilization, as the Fuegians, who use upon the body four colors of paint—red and yellow in token of friendliness, white as war-paint and black as a symbol of grief, and their neighbors the Patagonians who on the eve of the wedding night cover the body with white paint.

With the development of individuality there is corresponding development of variety in style in order to provide for each person a distinctive costume, but the human soul, too great and complex for its vestments, must go farther. Ordinary occasions do not provide scope for all its tendencies. Other selves within us must have their setting. In the pageant past times and personalities are rehabilitated and in the masquerade various conditions and occupations from which we are debarred actual participation, are for the moment, through the medium of clothes, made our own. In the masque also we may clothe our fantasies, moods and aspirations, the angel, devil, butterfly and flower within us each having its brief hour.

The era of distinctive national costume is passing. We are emerging from the dominance of blind impulse and entering upon the stage of preparation for a distinctly human garb. There are occasional efforts to break away from our modern complexity and elaboration and return to the noble simplicity of Greek attire,—the one perfect costume, as it has been called. But no one is able to forecast the future. The ultimate costume is as little predicable as the ultimate man.

Taken for
Friday May-5-'16.

A METHOD OF TESTING THE STRENGTH OF INSTINCTS

By Henry T. Moore, Dartmouth College

The over-rapid growth of mental tests during the past ten years has not infrequently raised the question whether psychology for the present can not more profitably confine its attention to standardization of the old than to encouragement of the new. But whenever one asks himself how to make mental tests of broad practical service to society, he is inevitably faced with a consideration which must dissatisfy him with the very limited reach of the tests at present available. Although much has already been accomplished in the differentiation of the feeble-minded, and much seems likely to be accomplished in the more scientific selection of employees for minutely specialized tasks, such as telephone operating and clerical work, still it is impossible to escape the conviction that such social needs as vocational guidance can never be greatly furthered by mere statistics as to mental acuity of various sorts in different individuals. Let us suppose, for example, that one man proves extraordinarily apt in all tests of perception, association, etc., but is found afterwards to be wholly lacking in curiosity regarding anything not connected with his animal needs; another man ranks only fairly well, or even below average, in the same tests, but possesses an all-consuming curiosity, which impels him to intense and sustained interests. It would surely be hazardous to predict for the first a larger measure of success than for the second. Indeed, it can hardly be expected that mental tests proper can ever be applied with conspicuous success to any positions which are not of a fairly routine character. The writer had occasion recently to ask a prominent physician to rate in order of importance for the general practice of medicine a large number of mental aptitudes, such as quickness of perception, logical association, rote and logical memory. The latter consented to make an estimate of the degree to which each was serviceable, but added finally that he thought that one might rank low in each of the tests, and yet find his way to the top of his profession; likewise, that one might rank high in each test, and yet make a decided failure. What is true of medicine is abundantly true of law, teaching, salesmanship, even of those occupations in which the worker is in every sense an employee. In nine cases out of ten more depends on the amount of motive force and on the ability to make successful social contacts than on the ability to perceive, remember, and imagine.

If the hope of psychology is ultimately to offer solutions for practical problems, its tests will have to be concerned more than heretofore with measures of instinctive and emotional tendencies. To be sure, it may seem somewhat untimely to attempt to measure instincts, when there is as yet nothing like universal agreement as to the specific marks of an instinct, but there is at least a fairly general consensus of opinion regarding the more important ones, such as pugnacity, fear, and curiosity; and for the rest, the very effort at quantitative measurement ought to lead in time to more precise characterizations which will prove generally acceptable.

I propose, then, to take as a starting point a classification of instinct very like the one which has been offered by McDougall in his "Social Psychology." We may limit ourselves to ten tendencies which are of great practical importance, and which are sufficiently specific to be termed instincts proper in his limited use of the term. We have, then, as traits to be measured: pugnacity, fear, repulsion, curiosity, self-assertion, self-abasement, tenderness, gregariousness, acquisition, and construction. If we can establish norms for each of these tendencies, and then test a given individual for an excess of pugnacity or for a deficiency in self-assertion, obviously we shall have made a beginning toward a kind of understanding, the social importance of which is not to be denied.

The method which has seemed to me most likely to display in their relative strength the above-mentioned tendencies, is a form of the association test. The subject is given as a stimulus some word strongly suggestive of a certain kind of emotional situation, and he is to react with the first verb form that occurs to him. The supposition here is that just in proportion as the emotion is frequently or strongly experienced by him, he will tend the more quickly and the more uniformly to respond with a verb reaction appropriate to the emotion suggested. Thus if one of the pugnacity stimuli, such as "enemy," "insult," or "attack," is given, a subject by nature very pugnacious may be expected to react with considerable speed, and with a verb-form indicative of resentment or attack. If one of the self-assertion stimuli is offered, such as "career," "success," or "achievement," a self-assertive individual may be expected to reply quickly, and with such expressions as "strive," "struggle," "attain."

Stated more in detail, the method of procedure is as follows. In a preliminary experiment the subject is given a series of twenty neutral words, such as "pencil," "leaf," "chair," etc., and is asked to respond with a verb expressive of his personal reaction to these objects. The average time for these twenty responses represents his normal verb reaction time. Instructions are now impressed upon him to the effect that in what follows he is to interpret each stimulus word as representative of a situation in which he is personally concerned, and that his reaction verb is to indicate an action in which he involves himself personally. With association controlled to this extent, he is supplied one at a time with one hundred stimulus words, by means of which each of the ten instincts is solicited ten times in irregular order. The subject's record for a given instinct will be an expression of the two factors of content and speed. In so far as it depends on content, it will be determined as follows. A reaction evidently provoked by the suggested emotion is scored by giving twelve points' credit to the instinct in question. If the stimulus brings forth a colorless word, which introspection proves to have been unaccompanied by any distinct emotional consciousness, the score for that reaction is zero. If the reaction expresses an emotion different from the one normally expected, as when a pugnacity stimulus yields a tenderness or fear reaction—such as "enemy-forgive," or "enemy-run"—the instinct of tenderness or that of fear is credited with the twelve points, and pugnacity with a zero. In these cases the overready instinct steals for itself extra credit at the expense of the weaker tendencies. It appears from the above that the total score for an instinct which provoked normal responses for each of its ten stimuli would be 120 points. The only variations possible, as far as the content of the reaction is concerned, are those which depend either on the number of neutral reactions, which are without score, or on the extent to which one instinct steals from another.

The score, as far as determined by speed, will depend on the extent to which the reactions connected with a given emotion are made in more or less than the normal verb-association time. For each increment of speed there should be a proportionate increase in credit, and for each retardation a proportionate decrease. Thus if the pugnacity reactions are made at an average of three-fourths the normal time, the total score for pugnacity, otherwise 120, now becomes 160. If the reactions average twice as long as normal, the score will total but 60 points.

Too much emphasis can hardly be laid on the need for giving careful consideration to the subject's introspective report on each reaction. This should be sufficiently detailed to make clear the following points:

(1) That the stimulus word is really interpreted as indicating a situation in which the subject finds himself personally concerned. It makes a considerable difference whether the word "danger" is interpreted as meaning my own personal hazard or the danger to the Mexican republic.

(2) That the verb is one which the subject seems to connect with his own activity. If the word "enemy" is replied to with "attack," it must be ascertained that the attacking is indicative of the subject's reaction toward the enemy, and not of his expectation from the enemy.

(3) That the reaction is not due to a chance association based on recent occurrences. The word "sports," which might normally call forth an expression of play, would, if recently associated with a serious accident, provoke an altogether different response.

Whenever the introspection points to the subject's failure in any one of the above three respects, it will of course be necessary to discard the results for that particular trial, and later in the test to introduce a substitute stimulus word. The introspective report will be further useful in determining whether the verb should be interpreted as colorless or as expressive of an instinct. For example, the word "is" implies emotional indifference when the introspection reveals nothing in particular. But when a subject reacts to "enemy" with "is," and adds, "Jones is my enemy, and I hate him," we have evidence of a pugnacity not adequately represented by the mere word form.

In calculating the score for fear and for self-abasement, exception will have to be made to the rule of deducting credit for slowness of response. The retardation for these two instincts may be due to the subject's natural disinclination to reveal his true emotion with complete abandon. Introspective evidence will have to be very carefully sought in these cases. A concrete instance taken from tests I have given will make clear this type of difficulty. One subject took twice the normal time to respond to "death" with "fear." Questioning elicited the following introspection: "My first thought was fear, but I tried to suppress this tendency, saying to myself that I would certainly not admit a fear of death. I was soon aware, however, that I could think of no other word for the moment, so I reacted with it." The time which elapsed during this unsuccessful attempt to suppress a fear reaction seems to indicate more rather than less of the instinct. A fear which reasserted itself in spite of attempted suppression would seem entitled to double credit. Moreover, an additional credit should be assigned in this case either to self-assertion or to self-abasement, according to whether the motive for suppression was the effort to control or to conceal the emotion. The former motive would justify an extra credit for self-assertion; the latter, an addition to the score for self-abasement. Obviously it is out of the question to attempt any of the refinements of psychoanalysis in giving tests of a routine nature; hence it may prove ultimately that the only way to uniform scoring lies in discarding all trials in which there is evidence of inhibition of the natural response.

The writer has recently begun a series of instinct tests on a large number of Dartmouth undergraduates, the results of which will be forthcoming shortly. For the present a list of fifty characteristic stimulus words, and the record of a single subject for these fifty words will indicate the method in detail

RECORD FOR A SINGLE SUBJECT

Preliminary Experiment

Neutral stimuli	Reaction	Time
cloth	cut	.6138 sec.
plate	eat	.5580
brush	brush	.4278
house	live	.6882
paper	cut	.5394
soup	eat	.5766
tree	see	.7254
pencil	write	.4278
chair	sit	.4836
bed	sleep	.8056
		10 | 5.8462
	Normal verb reaction time	.5846

Final Experiment (Words Given in Irregular Order)

Pugnacity stimuli	Reaction	Time	Introspection	Score
enemy	face	1.0418		7
insult	avenge	.8186		9
attacked	feel	.3720	Thought of fear	See fear
battle	fight	.5394		13
slapped	hurt	.8744		See self-abasement
		Extra credits taken from others		0
		Total score for pugnacity		29

Fear stimuli	Reaction	Time	Introspection	Score
danger	hear	.2976		24
death	fear	.8558		8
terror	feel	.5952		12
rattlesnake	fear	.7812		9
murder	help	.5022	Called for help	13
			Extra credits taken from elsewhere	16
			Total score for fear	82

Repulsion stimuli	Reaction	Time	Introspection	Score
stench	smell	1.358	Disagreeable imagery	6
putrid	smell	.8000	Disagreeable imagery	9
filth	hate	.7068		10
sores	hate	.8744		8
slimy	hate	.6326		11
			Extra credits	0
			Total score for repulsion	44

Curiosity stimuli	Reaction	Time	Introspection	Score
news	hear	.7626		9
strange	see	1.3580		6
novelty	see	.8558		8
puzzle	meet	.6138	Meant to say solve	12
interesting	like	.7440		10
			Total score for curiosity	45

Self-assertion stimuli	Reaction	Time	Introspection	Score
career	have	.4092	Thought of personal career	18
success	have	.3720	Could not avoid word "have"	19
power	have	.5022		14
triumph	have	.4278	Thought of attaining success	17
prestige	like	.7812		9
			Total score for self-assertion	77

Self-abasement stimuli	Reaction	Time	Introspection	Score
chagrin	feel	.6138		12
remorse	feel	.5952		12
disgrace	feel	.7440		10
guilt	feel	.8930		8
punishment	have	.8930	Thought of having been punished	8
			Extra credits	8
			Total score for self-abasement	58

Tenderness stimuli	Reaction	Time	Introspection	Score
mother	love	.5440		13
home	find	.9302		8
family	love	.6882		10
sister	have	.7068	Have always desired a sister	10
parents	love	.6326		11
			Total score for tenderness	62

Gregariousness stimuli	Reaction	Time	Introspection	Score
crowd	push	.8560		8
alone	hate	1.0790		7
friends	meet	1.02322		7
strangers	meet	.9860		7
solitude	hate	.8186		9
			Total score for gregariousness	38

Acquisition stimuli	Reaction	Time	Introspection	Score
wealth	get	.8370		8
property	have	.3906		18
possessions	have	.3348		21
money	get	.8186		9
valuable	have	.6326		11
			Total score for acquisition	67

Construction stimuli	Reaction	Time	Introspection	Score
enterprise	begin	1.0970	The word "have" seemed to force its way into every personal attitude	7
plans	hear	1.1348		7
vocation	have	.5952		12
system	have	.9488		8
policy	have	.6696		11
			Total score for construction	45

When a large number of such records have been obtained, it will be possible to make an approximate statement regarding a given individual's emotional nature, as compared with that of his fellows. Whatever the shortcomings of the method here outlined, the demand which it attempts to meet seems to the writer imperative. That economists feel the urgent need of an exact treatment of the instinctive-emotional life is abundantly evidenced by the increasing number of books such as Taussig's "Inventors and Money Makers," Veblen's "Instinct of Workmanship," and Anderson's "Social Value," which manifest in common the tendency to refer economics back to a psychology of instinct and emotion. It behooves the psychologist to supply the economist for this purpose with far more precise instruments than those which have been heretofore available.

MEASURES OF VARIABILITY

By E. J. G. Bradford, King's College, London

When the same test is applied several times the individual tested may vary at each application, in the efficiency of his response. The standard measures of variability,—the Average Deviation, the Standard Deviation, and the Coefficient of Variation,—can all be used to measure the degree of the individual's variability. But all these measures, however efficient they may be for measuring variability within a group of individuals, have serious disadvantages when applied to the variability of the individual.

Since all three measures suffer from the same defects, a consideration of the Average Deviation (A. D.) may serve as a basis for argument. In the first place the deviations are calculated from the mean performance. Continued application of the same psychological test enables the individual tested to improve in efficiency. Thus, for example, the mean performance at the last two applications will be higher than the mean performance at the first two. There is, in fact, a general improvement in the mean performance,—or stated differently, the mean is progressive.

A more exact measure of variability, then, would be one in which the deviations were calculated from a progressive mean instead of from a fixed mean.

The diagram below represents the scores of two individuals A and B at ten consecutive applications of a controlled association test, the exact nature of which will be described later.

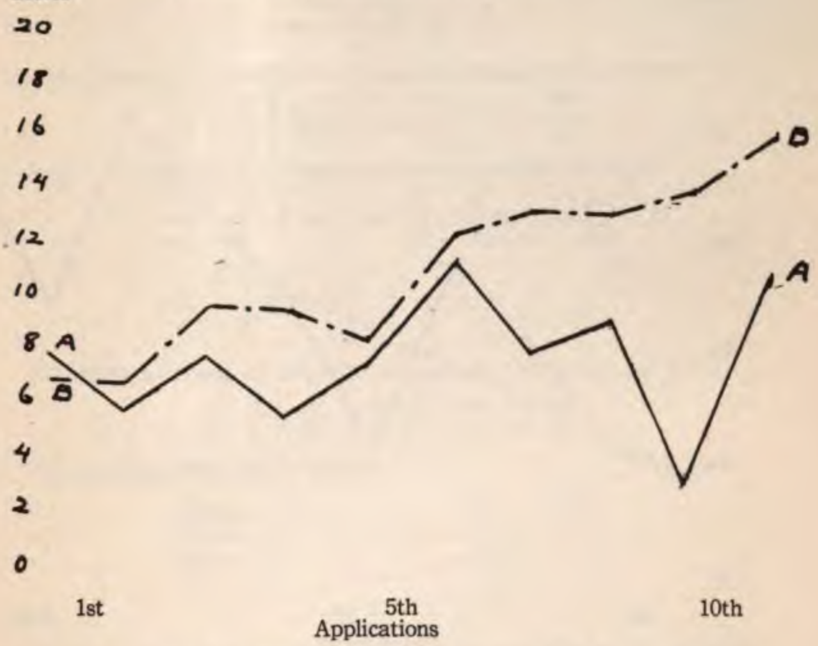
What strikes one on looking at the two series of results, is that subject A is more variable than subject B; for the line A certainly presents a far more irregular appearance than does line B, yet the means and A. D.'s of the two series are as follows:

A. Mean 8 [nearest whole number] A. D. 1.9
B. Mean 12 [nearest whole number] A. D. 3.0

Subject B shows a much greater improvement with practice than does subject A, and it is this improvement which results in such a large A. D. Herein lies the second great

DIAGRAM I

The scores of two subjects in the F. A. test



disadvantage of the A. D. If for psychological purposes we are to distinguish between variability and improvability, as seems advisable, the A. D. (or Mean Variation) must be discarded as a measure of individual variability. What is required therefore, is a coefficient of variability, which gives a measure of variability quite independently of the subject's improvability.

Consider the following case. A subject makes the following ten consecutive scores:

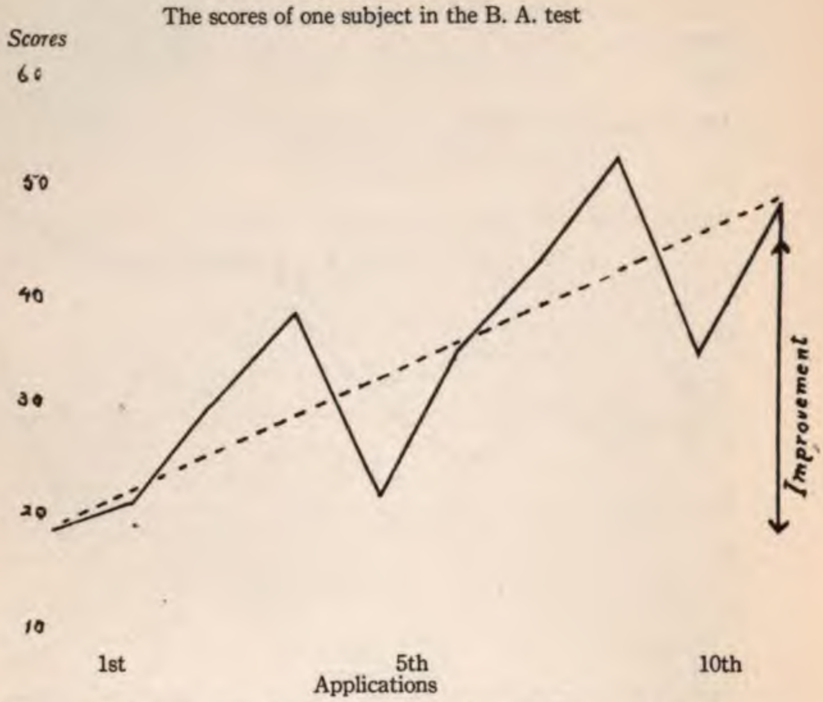
19, 21, 30, 39, 21, 35, 44, 53, 35, 49
(2) (9) (9) (18) (14) (9) (9) (18) (14)

The total variability can be measured by taking the sum of the differences (figures given in brackets above) between every two consecutive scores. In effect this amounts to reckoning the variations from a progressive mean.

It can be seen more plainly from the graphed results of these scores (Diagram II) that the sum of the differences

DIAGRAM II

The scores of one subject in the B. A. test



between the consecutive scores has been increased by the total difference between the initial and final means. In other words it has been increased by the improvement shown between the first and last measures. Hence to correct for improvability it will be necessary to diminish the total variability of the total improvement.

Let n be the number of measures.
m_1 and m_n the first and last measures respectively.
Then
$m_n - m_1$ is the improvement due to practice.
and

$$\left\{\left(m_n - m_{n-1}\right) + \left(m_{n-1} - m_{n-2}\right) + \ldots\ldots + \left(m_2 - m_1\right)\right\}$$

is the total variability due to all causes combined.

Putting
$m_n - m_1 = D$
$(m_n - m_{n-1}) = d$
We get
$(\Sigma d - D)$ as the total variability due to causes other than practice.

For a small number of measures, or at least such a number as shall be well within the limits of improvement, we can take as the measure of improvability (i)

$$i = \frac{m_n - m_1}{n} \text{ or } \frac{D}{n}$$

As a measure of variability including improvement (vi) we have

$$vi = \frac{1}{n}\left\{\Sigma\left(m_n - m_{n-1}\right)\right\} \text{ or } \frac{\Sigma d}{n}$$

Then it follows that as a measure of pure variability (v) we get

$$v = \frac{1}{n}\left\{\Sigma\left(m_n - m_{n-1}\right) - \left(m_n - m_1\right)\right\}$$

or,

$$v = \frac{1}{n}\left\{\Sigma d - D\right\}.$$

The actual scores of subjects A and B (Diagram I) were:

A. 8, 6, 8, 6, 8, 12, 9, 10, 4, 12.
B. 7, 7, 10, 10, 9, 13, 14, 14, 15, 17.

The coefficients work out as follows:

	A.	B.
Mean	8.0	12.0
A. D.	1.9	3.0
vi	3.0	1.2
i	.4	1.0
v	2.6	.2

From these two cases it would appear that the A. D. is more a measure of improvability than of variability. This problem as to the relations between the different coefficients will be dealt with in the sequel.

To show the relations between the various coefficients of variability and incidentally their relations to ability, certain experiments were made, the methods and results of which are given below.

The Subjects. Two classes of boys from a good elementary school in London were tested. Of these classes, in scholastic attainments, Class 4 was a year in advance of Class 5. In both classes the average age was 11.0 years, for Class 4 $\sigma = .60$ years and class 5 $\sigma = .95$ years. Hence, although of the same average age, the more advanced class was the more homogenous with regard to age. The number of boys who completed the whole series of tests were 43 in Class 4 and 45 in Class 5.

The Tests. The tests employed were the Backward-alphabet test and the Forward-alphabet test. (Hereafter referred to as the B. A. and F. A. tests respectively.) The former test needs no description in that it is described by Whipple[1] and has been used by other investigators.[2] The latter test differs from the former only in that the letter succeeding the stimulus letter is required instead of the letter preceding the stimulus letter.

Procedure. The tests were applied three times a week, on Mondays, Wednesdays and Fridays, for a period extending over about a month. The subjects were supplied with a paper on which twenty letters were printed in a vertical column. There were three different orders for the letters so that there was an interval of a whole week between each application of the same list.

At the commencement of this series of tests 70 seconds was allowed for the B. A. and 35 seconds for the F. A. test. The time was gradually cut down till at the end the times were 35 and 25 seconds respectively. The scores were weighed proportionally to the time given, in each case bringing them up to the 70 and 35 second standards respectively.

The Relation Between the Two Tests. The table below gives the correlation coefficients between the B. A. and F. A. tests, as regards mean ability, variability and improvability.

TABLE 1

	Mean	A. D.	*vi*	*i*	*v*
Class 4	.77	.24	.42	.34	.40
Class 5	.86	.35	.32	.32	[*.04*]

For Class 4:
$v = 3$ P. E. when $v = .29$
$v =$ P. E. when $v = .11$
For Class 5:
$v = 3$ P. E. when $v = .28$
$v =$ P. E. when $v = .10$

In the above and subsequent tables of correlation coefficients, those coefficients which are less than three times the P. E.

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[1] *Manual of Mental and Physical Tests.* 1910, 326.

[2] H. A. Aikins, E. L. Thorndike and E. Hubbell, *Psychol. Rev.* IX., 1902, 374 ff.

are printed in italics, and those which are less than the probable error are put in brackets.

It can be seen in the above table that there is a high correlation between the mean ability in the B. A. and F. A. tests. This is what might have been expected from the similar nature of the tests. Improvability (as measured by *i*), in the one test correlates with improvability in the other. The degree of correlation is not high but it is at least deserving of consideration since it is as great as three times the probable error; and it is approximately the same for both groups of subjects.

The coefficients obtained by correlating the variability (*v*) for the two tests shows that in Class 4 there is a marked tendency for variability in the B. A. test to be accompanied by variability in the F. A. test. On the other hand there is a marked absence of a similar correlation for Class 5.

Since both the *i* values and the *v* values show an appreciable degree of correlation between the two tests for Class 4, this would be sufficient to account for the fact that the *vi* values give a higher correlation than either of the other two separately. For Class 5 the *vi* values do not give greater correlation, this may be due to the fact that the *v* values for this class show practically no correlation at all.

The Relation between the Means and A. D.'s.—Table 2 shows that a high correlation obtains between the A. D. and the Mean in both tests and for both classes.

TABLE 2

	Correlation between Mean and A. D.	
	B. A.	F. A.
Class 4	.63	.50
Class 5	.40	.59

Does this high correlation mean that, generally speaking, variability is a function of ability; that the more able an individual is, the more variable he will be?

An answer to this question will be found in Table 3, which gives the correlation coefficients between the other measures of variability and improvability.

TABLE 3

	B. A. test			F. A. test		
	Correlation between Mean and:—					
	vi	*i*	*v*	*vi*	*i*	*v*
Class 4	[*.03*]	.48	—*.24*	[*.07*]	.38	—*.11*
Class 5	*.23*	.33	[*.04*]	.43	.74	[*.00*]

It is obvious that the answer to the above question is in the negative. For these two tests and for these two groups of individuals there is no marked tendency for variability to correlate with ability, either positively or negatively. None of the coefficients are as great as three times the probable error. The apparent exception to this generalization is found in the correlation between the Mean and *vi* for Class 5 in the F. A. test. This positive correlation is probably accounted for by the fact that the improvability factor which the *vi* coefficient contains, itself correlates very highly with the Mean.

The second point of note in this table is that in all four cases ability correlates with improvability; all four coefficients are greater than three times the P. E.

The Relation of Variability and Improvability with the A. D. A question which arises from the above considerations is, why should the A. D. correlate highly with the Mean, while *vi* and *v*, the other measures of variability do not?

TABLE 4

	B. A.			F. A.		
	Correlations between A. D. and:—					
	vi	*i*	*v*	*vi*	*i*	*v*
Class 4	*.16*	.71	—.31	.47	.66	*.12*
Class 5	.58	.61	*.24*	.52	.59	*.17*

The high correlations between A. D. and *i* and the low correlations between A. D. and *v*, show that the A. D. is not a measure of variability in the limited sense in which the term here is used. The A. D. is far more a measure of improvability than of variability.

A complete table of the correlation coefficients between the various measures and the Mean is given below.

TABLE 5

	Coefficients	B. A.		F. A.	
		Mean	A. D.	Mean	A. D.
Class 4	A. D.	.63	...	.50	...
	i	.48	.71	.38	.66
	vi	[*.03*]	*.16*	[*.07*]	.47
	v	—*.24*	—.31	—*.11*	*.12*
Class 5	A.D.	.40	...	.59	...
	i	.33	.61	.74	.59
	vi	*.23*	.58	.43	.52
	v	[*.04*]	*.24*	[*.00*]	*.17*

The Relation Between Variability and Improvability. The fact that the *vi* value correlates less highly with the mean value than does the *i* value, suggests that the variability factor in the *vi* coefficient acts in opposition to the improvability factor. This view is well substantiated by the correlation coefficients between the *i* values and the *v* values. Generally speaking, it would appear that the greater a subject's improvability the less would be his variability.

TABLE 6

	Correlations between *i* and *v*	
	B. A.	F. A.
Class 4	—.35	—.61
Class 5	—*.24*	—.28

CONCLUSIONS

1. The A. D. of a series of performances by the same individual, is not a measure of variability but rather one of improvability.

2. A relation of a positive nature exists between ability and improvability.

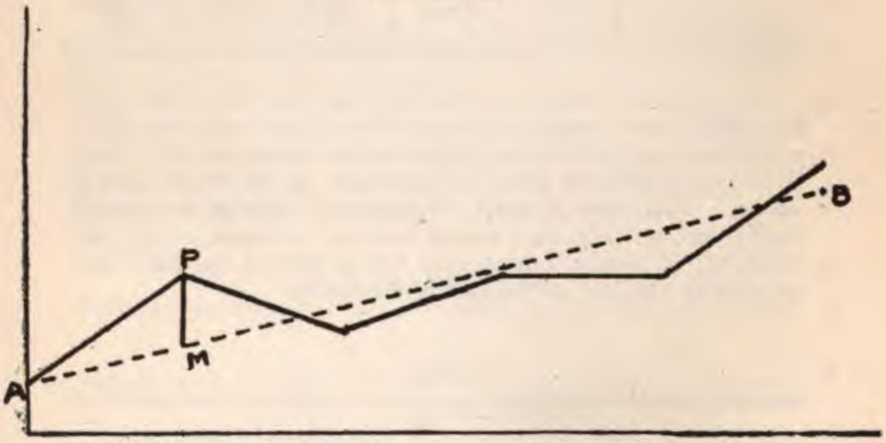
3. No such relation exists between ability and variability (v values) of performance in the two tests used.

4. The relation between variability and improvability is of a negative nature; that is, so far as these two groups of subjects and these two tests are concerned.

APPENDIX

An objection to the measures of variability suggested in this paper, is that they do not fall into line with those used in statistical work. To meet this objection Dr. W. Brown has made out a formula "to determine the true value of the M. V. of a series of performances of a single individual, when the performances show a progressive improvement."

DIAGRAM III



Let AB be the best fitting straight line, its equation being $g = mx + c$. Then the true M. V. will be the average of all such distances as P. M., taken without regard to sign.

Put $PN = Y$, $MN = y = mx + C$

Using the method of best squares we get as the best fitting line,

$$y = \frac{12S(Yx) - 6(x+1)S(Y)}{x(x^2-1)} . x_x + \frac{(4x+2)S(Y) - 6S(Yx)}{x(x-1)}$$

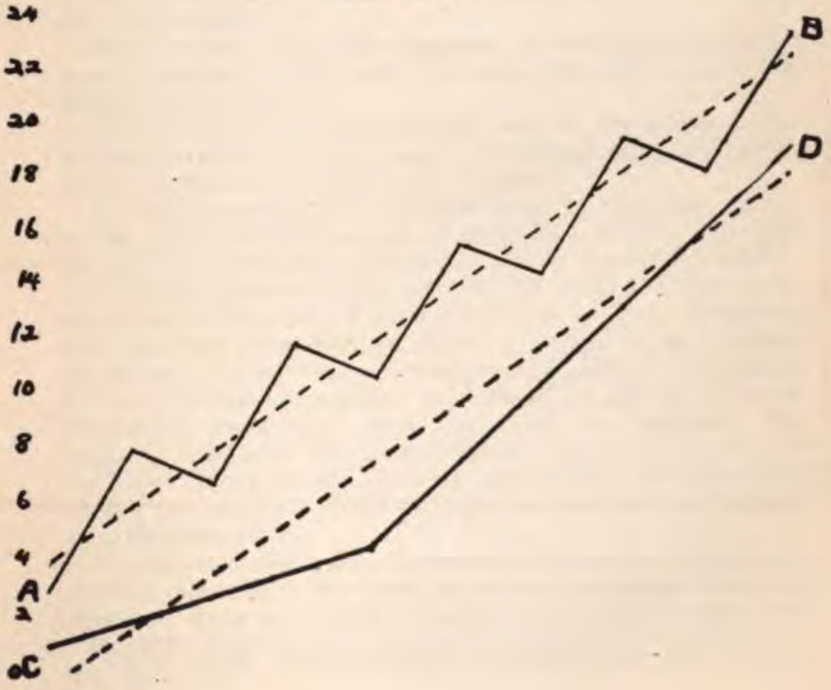
where x_x is the current coordinate.

The values of the successive ordinates of this line are determined by substitution; then the average of the differences between these ordinates and the corresponding ones of the curve (taken as all positive) will be the true value of the M. V.

Dr. Brown's "true M. V." is however a measure of the change of *the rate of improvement,* as well as of variability between consecutive performances (considered apart from improvement).

The two imaginary cases given below show this difference. The Curve CD shows change in the rate of improvement. The curve AB shows variability between consecutive performances.

Diagram IV



The performances represented by the line AB give a "true M. V." of 1.3 and $v = .8$. From the curve CD the "true M. V." works out to 1.3 and $v = O$.

In the case of the line AB the "true M. V." measure the variability between consecutive performances only. The "true M. V." of the curve CD is a measure of the change in the rate of improvement.

The "true M. V." of each of the 45 subjects for the B. A. test (Class 5) was calculated and the results were correlated with the other measures with the following results:

CORRELATION COEFFICIENTS BETWEEN "TRUE M. V." AND:—

v	i	vi	Mean	A. D.
.48	.37	.74	.34	.58

THE CAUSAL RELATIONS BETWEEN STRUCTURE AND FUNCTION IN BIOLOGY

By E. Stanley Abbot, McLean Hospital, Waverley, Mass.

In a not very wide search through the possible literature the writer has been unable to find a solution of the problem indicated by the title of this paper. That he is not the only one to whom it has been more or less of a puzzle is evident from some comparatively recent discussions in medical and other societies. In one it was said " The question of the relation of structure to function is the great biologic problem," the speaker stating that " function determines structure just as in its turn structure determines, delimits—that is, places definite limits on function," [1] but without further elucidating the relationship.

Does structure determine function, or does function determine structure, or does each determine the other, and if the latter, how can it be?

The answers to these questions seem to the writer to be matters partly of definition, partly of point of view, and partly of the recognition of purpose in Nature.

In the inorganic world structure may be regarded merely as the mechanical collocation of parts in a whole. The pile of sand has structure in this sense. But in the biologic world, and in all structures made or used by man, structure is a mechanical collocation of parts *adapted to an end.* Structure then becomes a machine, an organ, a mechanism as a means to an end. Though there are some who still try to explain all vital (including psychic) phenomena on a basis of mere mechanical cause and effect, and think they succeed, the majority hold with Kant that efficient causes alone are not sufficient to explain the facts of organic life, and that final causes are essential to the explanation and that they belong to a different series.

In the so-called exact sciences—mathematics, chemistry, physics, astronomy—the question of end and means does not arise; or if, as in applied physics, it does arise, it takes the form of man's devising means to attain his own definite conscious ends or purposes.

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[1] Dr. W. Ophüls, Anatomic Structure and Function, *Jour. A. M. A.,* Vol. 63, p. 521.

But in the biological sciences it is not so obvious who conceives the end that is to be served. Personally the writer feels forced to the conclusion that all Nature—the universe as a whole—is a personality whose total end is the perfection of all its parts; that to this end the development in each evolving part, or biological unit, of mechanisms which enable this part to adapt itself better and better to its environment is an essential means; and therefore that it is Nature itself—the universal personality—which has conceived the ends which are served by biological mechanisms, whether physical, psychical or ethical.

Not only universal ends but also those of the biological unit are served by these mechanisms. But it was not the individual unit that conceived the ends to which its structure is adapted. Having the structure, however, the individual may use it for its own conscious ends, where evolution has advanced far enough for it to formulate them. So rarely in biology do we see structures that are not adapted to ends, that when we happen upon a new structure we inevitably ask what its purpose may be. As a matter of course we assume that it serves *some* purpose. Hence we are justified in defining structure in biology as *a mechanical collocation of parts adapted to an end.*

In the biological sciences the word *function* is used in connection with the *activity* of some structure or mechanism; and, since structure or mechanism involves an end, function has to do with *activity directed to an end.* This expresses an actual or potential relation between two terms, a something which acts or is capable of acting, and a something which is done or is to be done. This relation may be regarded from the point of view of one term or of the other—actively from the point of view of the doer, i. e., structure, *agens,* or passively from that of the thing to be done, *agendum.*

When, for example, we say " it is the function of the stomach to digest food," we use the word from the point of view of the stomach as agent, *agens.* In this case function means *activity directed to an end,* and it expresses the relation between the terms—the doer and the end.

But when we say " food is to be digested, and that is the function of the stomach," we use the word from the point of view of the thing to be done, *agendum.* In this case function means *an end toward which an activity is directed*, and it therefore expresses but one term of the relation.[2]

In many instances it is immaterial which meaning of function we have in mind—the context makes us automatically choose the appropriate one. But when the two meanings are brought into contrast, with only the one word to use for the two meanings, as in considering the causal relationships between structure and function, confusion inevitably arises, unless we keep clearly in mind that there *are* two meanings. With this difference in mind we can approach the problem of whether structure determines function, or vice versa, or if both are true.

Structure being a mechanical collocation of parts adapted to an end, it is obvious that it cannot act, i. e., serve that end, unless it already exists. The stomach must exist before it can digest food, i. e., perform its function in the sense of activity directed to an end. The structure of the stomach, whether a mere body cavity as in the "gastrula" or the highly complex organ of some of the higher animals, will determine the way in which food is digested. That is, in the actual performance of its function, the structure of the organ will determine how the thing is done. *Mechanically,* therefore, that is, in the cause-and-effect series, *structure* precedes, or *determines,* or is a cause of *function.*

But on the other hand, it is the end toward which an activity is to be directed (*agendum*) that will determine what sort of a mechanism is required. If a tree is to be felled, we do not attack it with a pencil, but with something—axe or saw—that will tear or cut its tissues. It is the nature of the food stuffs to be digested that determines the character of the organs and processes necessary to digest them. If our food had been iron, and oxidation necessary for its use, the structure of our stomachs would have been of such a character as to oxidize iron. In other words, in the development of organs, structure, or mechanism, for the performance of certain functions, the nature of the thing to be done, i. e., the function in the sense of end to which an activity is directed, will determine to some extent the structure. *Teleologically,* therefore, that is, in the end-and-means series, *function* precedes, or *determines,* or is a cause of *structure.*

When Spencer says "Everywhere structures in great measure determine functions; and everywhere functions are incessantly modifying structures," [3] he states a seeming paradox which is easily soluble if we paraphrase it, in accordance with the above distinction, "Everywhere mechanical collocations of parts adapted to ends in great measure [mechanically] determine the activities directed to [those] ends; and everywhere ends toward which activities are directed are incessantly [teleologically] modifying the mechanical collocations of parts adapted to [those] ends."

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[2] For the needs of the present argument it is not necessary to subdivide the ends as Ruckmich does (*Am. Jour. Psychol.,* Vol. 24, p. 99 sq.).

Thus it is seen that each proposition is true, but in a different sense. It is only teleologically that function determines structure, and only mechanically that structure determines function. In neither case is function or structure the *sole* cause of the other. And in no case can either structure or function or the relations between them be wholly understood on mechanical or on teleological grounds alone.

The value of a principle lies in its application. The application of the principle involved in the foregoing discussion has been of fundamental value to the writer (and hence it may be to others) in approaching the problem of mind, the subject-matter of psychology which he regards as one of the biological sciences—biology being conceived as the science of living things rather than as the science of matter in the living state.

So far as our experience goes mind is manifested only by living things, and by them as internal activities or reactions of individuals. These reactions are made in the course and for the purpose of the self-adaptation of the individual to his environment. The nature of the environment then will partly determine, as final cause, what kinds of activities the individual shall be capable of, i. e., what general types of reaction he must make, and hence the structures and mechanisms for making them.

This is obvious when we regard such reactions as those of seeing, hearing, etc. The physical nature of light and color as a mode of energy has determined, as final cause, the general nature of the organ which shall be sensitive to or stimulated into activity by it. Equally obviously the actual structure of the organ sensitive to light in any given biological unit, whether the red spot of the star fish or the eye of man with its nervous connections, will determine, as an efficient cause, the extent and completeness of the reaction made by that unit. Since every individual is finite, no reaction can be absolutely complete.

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[3] Principles of Biology, Vol. II, p. 4.

It is less obvious, though in the writer's belief not less true, that there are in the environment not only things, but *kinds* of things, the actual kind consisting of *all* the past and present individuals of the kind—all the specimens of the species. The existence of kinds as actual objective realities or factors in the environment is a determinant, as final cause, of the capacity of the individual to form *concepts,* and hence of the structure which subserves this function. The mechanism of this function is still obscure; the structure or organ is doubtless some part or parts of the brain. Since no individual can experience all the specimens of any given kind of thing, the concept which he forms will be in part determined, as efficient cause, by the number and variety of the specimens that he has experienced, and in part by the extent of his capacity to react in this particular way; this capacity being in turn efficiently determined by the structure of the brain. Our concept of any particular kind of thing or event grows or is modified by additional experiences of things or events of that kind, up to the point of our capacity to enlarge or modify our concepts. The feebleminded person has very limited capacity, the scientific genius very great capacity, in this respect.

Similarly, in the writer's belief, it is equally true that there are *relations,* not only spatial and temporal, but of many other kinds; and that there are many types or kinds of *law* or *necessity;* and many kinds of *obligation* or *duty;* and that because these are all actual objective realities existent in the environment, they have been final causes of the development of mechanisms and structures for the purpose of enabling the individual to adjust himself to them as factors of the environment.

There is very greatly needed, for psychology especially, a study of the environment, not only as a particular efficient cause for individual reactions of given types in given situations and circumstances, but as a general or final cause for the evolution in the individual of the capacities to react in all the ways in which it does react and of the mechanisms and structures for this purpose. For example, for us human beings obligation is a very important factor in our environment, in consequence of which a capacity for forming moral concepts has been evolved in us. What types of obligation are there? To which of them does this or that person react, or can he react? Can or do any of the lower animals react to them, and if so, to which? Have any mechanisms been evolved in them for this purpose?

If we would fully understand comparative psychology and animal behavior, we must know what factors of the environment the animal is capable of reacting to, and what the *whole* situation is to which it is reacting, as well as the reaction itself in any given experiment or observation. The same is true of human psychology and behavior. In the study of pathological psychology the factors which affect the structures that subserve these functions and modify their mechanisms must of course also be considered; in normal psychology they may be largely taken for granted. It may be added that a comprehensive study of the nature of the environment—an ontology—must of necessity lie at the foundation of a thoroughly scientific theory of knowledge, and becomes essential if we would fully understand some of the differences between knowledge on the one hand and ignorance, error, delusion and hallucination on the other.

A NOTE ON COLOR PREFERENCE

By M. Luckiesh, Cleveland, Ohio

During the course of investigations in color science some data relating to color preference were obtained which will be recorded briefly. The Wundt (E. Zimmerman) colored papers were used but as there was no saturated green paper such a one was dyed and placed in the series. This color is designated as q, the other letters in the illustrations indicating the catalogue designations of the various colored papers. Fifteen colored papers each four inches square were spread out haphazardly upon a white surface, the individual colors being from six to ten inches apart. The observer was instructed to isolate the colors from everything as far as possible and to choose them in their order of preference for 'color's sake' alone. As the colors were chosen they were laid aside. One of the objects of the investigation was to ascertain if there was an appreciable difference in the order of preference when the colors were chosen under ordinary artificial (tungsten) light, T, and under daylight, D. (It is well known that the spectral character of an illuminant greatly influences the appearance of a colored object both as to hue and relative brightness.) The experiments were therefore carried out under the light from incandescent tungsten lamps operating at 7.9 lumens per watt and also under daylight from the blue sky as it entered a large window. In both cases the intensity of illumination was sufficiently high for examining colors with ease.

Fifteen observers were used for both sets of observations, the personnel being the same in both series. The time elapsing between the two tests for individual observers varied from one to four weeks. The colors were ranked from one to fifteen and the mean ranks for the individual colors (obtained by averaging the individual ranks of the fifteen observers) are plotted in Fig. 1. There may be some question regarding the legitimacy of this definition of color preference but the procedure adopted here appears to be satisfactory in this case and certainly provides a simple method for plotting the data. It is to be regretted that more observer available but the data appears to lead to fairly definite conclusions. Lemon-yellow was placed last or next to last by a large majority of subjects as is evident by the fact that its mean rank is just above fourteen for both series. As seen in Fig. 1, deep blue ranked highest in the mean preference order although red-purple and three of the reds also ranked high.

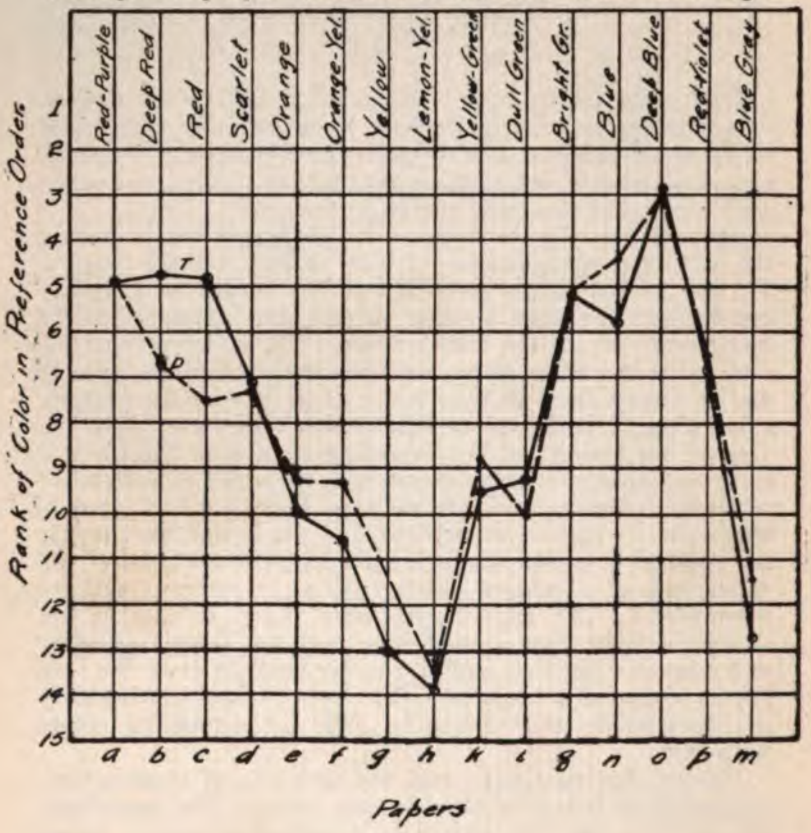

Figure 1

It is not surprising to find the reds ranking higher under the artificial illuminant (solid line) than under daylight (dashed line) although there appears to be little difference in the positions of the remaining colors in the two orders of preference. The latter may be accounted for possibly by the fact that the colors were quite saturated with the exception, *m*, on the extreme right, namely blue-gray. This color ranked quite low.

The colors have been arranged in the spectral order of their dominant hues as far as possible. In general the colors whose dominant hue lie near the ends of the spectrum are highest in the preference order. Such a result is consistent with conclusions drawn from other observations.

In Fig. 2 are plotted the mean deviations from the mean for each test color. A considerable variation is expected in such work and only when this variation is not too great is it advisable to draw definite conclusions. At the lower part of Fig. 2 the mean deviations are given in terms of units of rank—there being 15 test colors and therefore 15 units of rank. A fair average mean deviation is somewhat more than two units of rank. The variations do not appear to be sufficient to endanger the conclusions drawn. In the upper part of Fig. 2 the mean deviations are expressed in per cent. Where the rank is high obviously the per cent. deviation is higher owing to the fact that the actual mean deviations as expressed in units of rank are approximately constant.

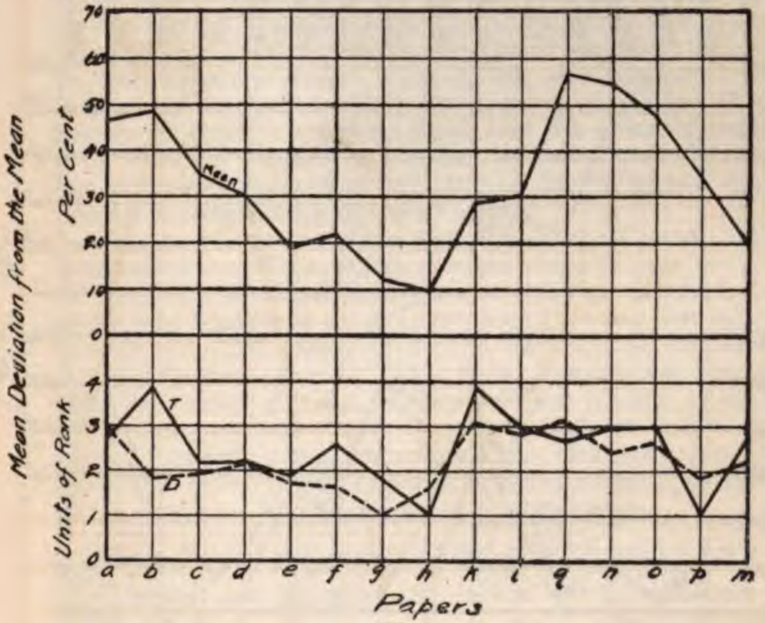

Figure 2

In Fig. 3 are plotted the reflection coefficients of the fifteen test colors for the two illuminants used. These of course represent also their relative brightnesses as viewed under illuminations of equal and uniform intensity from the two illuminants. It is seen that the preference of a given saturated color is lower as the relative brightness of the color is higher. In Fig. 3 are also plotted the ratios of the brightness of each color under the artificial light to that under the daylight. Owing to the variation of daylight in spectral character and intensity it is advantageous to use "artificial daylight." This the writer[1] has found very convenient for investigations in color.

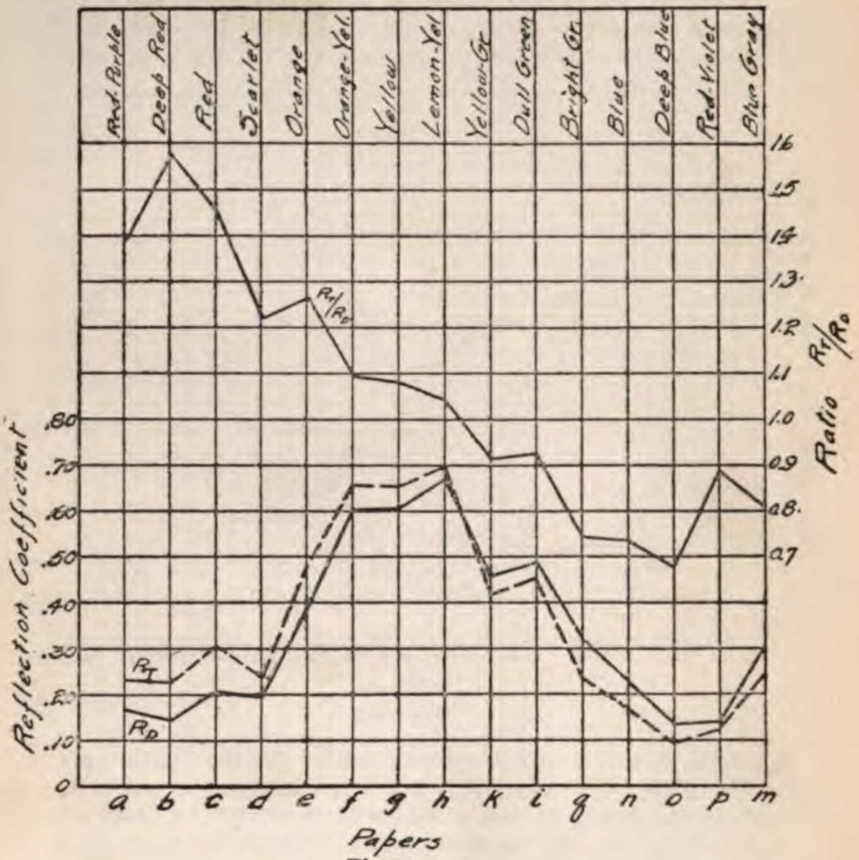

Figure 3

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[1] M. Luckiesh, Artificial Daylight, *Electrical World*, Sept. 19, 1914. M. Luckiesh and F. E. Cady, *Trans. I. E. S.*, No. 8, 1914, p. 839.

According to E. B. Titchener there are two types of observers; one type prefers the saturated colors and the other definitely prefers unsaturated or 'artistic' colors but the former type constitutes a majority. It appears to the writer from these and other observations that, when colors are chosen for 'color's sake' alone, the saturated colors are almost invariably chosen. E. J. G. Bradford[2] in experimenting with twenty-six students with a set of fifteen papers, each about thirty inches square, found that saturated colors were most preferred. He also found that the admixture of a small proportion of another color lowered the position of the color in the preference order. Cohn[3] has also contended that increase of saturation raised the position of a color in the preference order. Bradford found that the order of preference remained reasonably constant by performing the same experiments on three observers after an interval of two weeks and again after a lapse of twelve months.

It appears safe to conclude that, in a group of rather highly saturated colors, those, whose dominant hues lie near the ends of the visible spectrum, whose brightnesses are relatively low and whose saturations are relatively high (low per cent. white) are found to rank relatively high in the order of preference. Apparently the reds rank higher under incandescent tungsten light than under daylight. Not a sufficient number of colors of various tints and shades were used to draw further conclusions regarding the influence of the illuminant upon the preference order, however such an influence exists to an appreciable degree as is evident from daily experience.

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[2] On the Relation and Aesthetic Value of the Perceptive Types in Color Appreciation. *Amer. Jour. of Psych.*, 1913, 24, p. 545.

[3] Gefühlston and Sättingung der Farben. *Phil. Stud.*, 1900, 15, p. 279.

INITIAL SPURT IN A SIMPLE MENTAL FUNCTION

By J. Crosby Chapman and Willis J. Nolan, Western Reserve University

In a recent article[1] on initial spurt one of the writers intimated that this subject was being further investigated using shorter intervals of time. This paper records the result of that investigation. As was pointed out in the paper, to which reference has been made, the weakness of previous experiments on initial spurt has been the use of successive intervals of time of too long duration to reveal the rapid decline in efficiency which takes place. Consequently the effect of the spurt has been so neutralized as to escape attention. The former study in which two-minute intervals were used showed undeniable signs of initial spurt, but it appeared that the effect would have been more evident if shorter intervals had been employed. For this reason, in the present study it was thought advisable to resort to intervals of thirty seconds. This interval was judged the minimum which would not interfere with the freedom of the subjects in their work.

The general method of the experiment was similar to that already described. The subjects were tested in continuous addition for sixteen minutes on seven successive occasions. The addition blanks consisted of forty-eight columns of ten one-place numbers, and were similar to those used by Thorndike[2] and other experimenters. Five different sheets in all were employed so as to eliminate any memory effect. The experiment was conducted as follows. The sheets were supplied to the subjects face down. At the word, " Commence," the subjects reversed these sheets, and commenced to add down the columns. At each thirty-second interval the word, " Check," was called at which time the subject made a small tick on the right-hand side of the column on which work was being done. This mark indicated the exact place in the column which the subject had reached. The checking very soon became automatic, and was practiced before the data of the research were collected.

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[1] Chapman, *Jour. Educ. Psych.*, Vol. 6, Sep. 1915.

[2] Thorndike, *Amer. Jour. Psych.*, Vol. 21, 1910, p. 483.

Obviously in an investigation of this kind great care has to be taken that the subjects do not add in advance of the experiment. This was eliminated not only by keeping the sheets reversed before the experiment started but also by announcing the number of the column on which to commence work just previous to the starting signal. The subjects were not informed that the results would be used to investigate the problem of initial spurt. The tests were administered a week apart to twenty individuals who took the tests on the average seven times. The subjects were girls of an average age of seventeen, attending a private school in Cleveland. Each week the results of the previous tests were announced with the object of maintaining interest.

The basis of scoring was to allow ten for each column correctly added, since there are ten additions in each problem. No allowance was made for the time spent in writing the sum at the base. For each column in which an error was made ten was deducted. This possibly inflicted too high a penalty for errors, as in a normal case not more than one or two errors would be made in the same column. It seemed desirable however to inflict this penalty as it presents the only perfectly safe scoring basis. In this way it is possible, counting

TABLE I

AGGREGATE SCORES DURING EACH SUCCESSIVE HALF-MINUTE PERIOD
Fifteen minutes' continuous work on each day (Periods 1-30)
Twenty subjects tested on seven days

Half-minute period	Total uncorrected score	Total errors	Total corrected score	Half-minute period	Total uncorrected score	Total errors	Total corrected score
1	2858	582	2276	16	2176	453	1723
2	2511	476	2034	17	2234	404	1830
3	2363	575	1788	18	2125	389	1756
4	2251	437	1814	19	2262	413	1849
5	2274	446	1828	20	2152	543	1609
6	2254	337	1917	21	2247	463	1784
7	2315	419	1896	22	2195	443	1752
8	2192	440	1752	23	2242	488	1754
9	2349	462	1887	24	2232	384	1848
10	2221	511	1710	25	2298	478	1820
11	2258	417	1841	26	2290	504	1786
12	2183	498	1685	27	2185	452	1733
13	2140	418	1722	[illegible]	[illegible]	438	1798
14	2141	436	[illegible]	[illegible]	[illegible]	[illegible]	1810
15	2174	414	[illegible]	[illegible]	[illegible]	[illegible]	1782

each separate addition as a unit, to determine the exact amount of work done during each successive half-minute period.

The aggregate scores of all the individuals tested on the seven occasions in each successive half-minute period are shown in Table I. It will be seen that only thirty half-minute periods are recorded in order to eliminate any possible so-called end-spurt effects. The number of errors during each of these successive half-minutes is also recorded from which the corrected aggregate scores of each of the half-minute periods were obtained.

From Table I it will be seen that both the uncorrected and the corrected scores show a considerable initial spurt. This is markedly true in the case of the first period, but even in the second period it is still apparent; the efficiency during the second period never being equalled during any later period. In Table II the uncorrected and corrected scores of each successive half-minute are reduced to a percentage basis, in each case the product of the first half-minute being arbitrarily chosen as 100%.

TABLE II

Scores Reduced to Percentage Basis
(Score during first period = 100)

Half-minute period	Uncorrected score	Corrected score	Half-minute period	Uncorrected score	Corrected score
1	100	100	16	76	76
2	88	89	17	78	81
3	83	79	18	74	77
4	83	80	19	79	82
5	79	81	20	75	71
6	79	85	21	78	78
7	79	84	22	77	77
8	81	77	23	78	77
9	77	83	24	78	82
10	82	75	25	80	80
11	78	81	26	80	79
12	79	74	27	77	76
13	76	76	28	78	79
14	75	77	29	80	80
15	76	77	30	76	78

Table II only serves to bring out more clearly the facts recorded in Table I. A graphic representation of Table II is shown in Figure I.

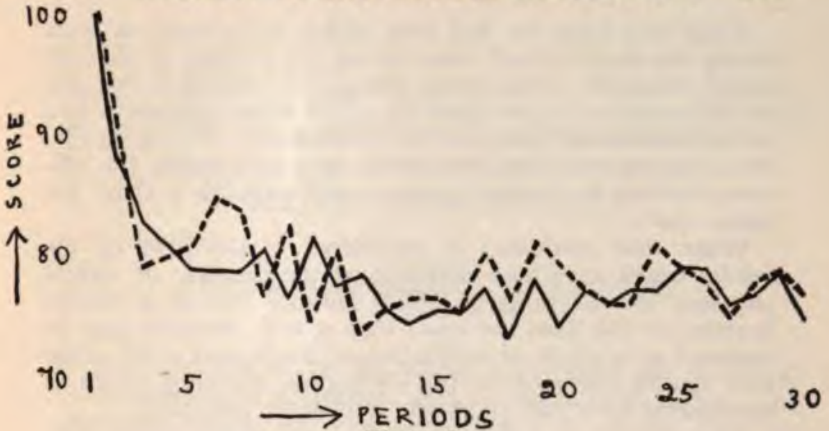

Figure I

In this figure the uncorrected score is represented by the continuous line, and the corrected score by the broken line. It will be seen that there is a large decline in efficiency in the curves representing both the corrected and uncorrected scores.

Perhaps the most advantageous method of presenting the results of this experiment is to compare the score during each early half-minute period with the average score of the last twenty half-minute periods. This has been done in Table III which shows the percentage amount by which each of the first ten half-minute periods exceeds the average of the last twenty half-minute periods in the case of both the corrected and uncorrected scores.

TABLE III

Average scores (periods 11-30) is exceeded by score during period shown below by percentage recorded in Columns I and II	Column I Uncorrected score	Column II Corrected score
	Percentages	Percentages
1	28.8	28.8
2	13.1	14.7
3	6.3	1.1
4	6.3	2.3
5	1.4	3.4
6	2.3	8.5
7	1.4	7.3
8	4.5	—1.1
9	—1.4	6.8
10	[illegible]	—3.4

From this table we find that 28.8% more work is done during the first interval than during the average of the last twenty intervals. Considering the second interval in the case of the corrected score there is 14.7% more efficiency, and the corresponding figure for the uncorrected score is 13.1%. Even leaving the first interval out of consideration the efficiency during the second period would establish a claim for initial spurt.

When this conclusion is considered in the light of the results which have been obtained on the decrease of mental efficiency due to fatigue,[8] which indicate that in a mental function of this kind not more than a 10% decrease may be expected as a result of twelve hours' continuous work, some idea can be obtained of the decline in efficiency which is represented by a fall of 28.8% in product produced.

An examination of the corrected scores of the twenty individuals taking part in the test is instructive. In the case of 80% of the subjects the total scores of the periods 1-5 exceeded the scores of the period 26-30. A comparison of the totals of the periods 1-3 with the totals of the periods 11-13 shows that 90% of the individuals were more efficient during the first three periods.

Although the errors during the first period are greater than during any successive period, arithmetical analysis shows that there is no method of penalizing the errors so as to reduce initial spurt to zero in the corrected score.

In view of the fact that the use of shorter intervals has revealed a more pronounced initial spurt effect, it does not seem possible to deny the presence of this factor. The subject when set to mental work of this type, owing probably to absence of interference, commences at a speed which is greater than he can maintain. He very rapidly settles down to a normal rate at which he can work for long periods. It is the rapidity of this decrease in efficiency which has led to the fact that initial spurt has been overlooked and even denied.

Our best thanks are due to the authorities of the Laurel School, Cleveland, for their courtesy in allowing facilities for this work, and also to Miss M. E. Hills for her assistance in conducting the class experiment.

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[8] Hollingworth, *Psych. Review,* Vol. 21, No. 6, p. 473, Nov. 1914.

THE EFFECTS OF PRACTICE IN ITS INITIAL STAGES IN LIFTED WEIGHT EXPERIMENTS AND ITS BEARING UPON ANTHROPOMETRIC MEASUREMENTS

By SAMUEL W. FERNBERGER, Clark University

In an earlier study[1] we found, by treating our results by the coefficient of divergence, that there was some factor which changed during the experimentation. A further statistical treatment, as well as the fact that the influence of this factor was greater upon a subject who was untrained at the beginning of experimentation than upon a trained one, led us to believe that this influence was due to the effect of the progressive practice which the subject obtained during the experimentation. We also found that the influence was greater at the beginning of the experiment.

Urban[2] takes up this problem by a consideration of his earlier results in lifted weight experiments, as well as a consideration of my results. Urban calculates the results which were taken according to the procedure of the method of constant stimuli in groups of 50 judgments upon each comparison pair. He studies the effect of this progressive practice upon the coefficients of precision (the *h*'s of the psychometric functions), upon both the upper and lower thresholds, and upon the values of the interval of uncertainty and the point of subjective equality. He finds that the values of the *h*'s for both the greater and the less judgments tend to increase. This increase is, in the early stages of practice, at first very rapid, and then proceeds relatively more slowly. The size of the interval of uncertainty tends to grow smaller under the influence of progressive practice. This is due to the fact that the thresholds, the values which limit this interval, tend more nearly to approach one another. The influence is not evenly exerted upon the two thresholds, however. The threshold in the direction of decrease remains relatively constant; the threshold in the direction of increase, on the other hand, tends to become smaller. Inasmuch as this change of the size of the threshold values is greatest for the upper threshold, there is also a change in the size of the point of subjective equality. This value tends to become smaller during the later stages of progressive practice. Urban also points out that the rate of change corresponds rather closely to the law of mass-action in chemistry. Hence the rate of improvement due to the practice obtained during a given interval is dependent not only upon the work performed during that interval but also upon the state of practice of the individual at the beginning of the interval.

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[1] S. W. Fernberger, On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli, *Psychol. Monog.*, XIV, No. 4 (Whole No. 61), 1913. 10-46.

[2] F. M. Urban, Der E [illegible] wichtsversuchen, *Arch. f. d. ges. Psychol.,* [illegible]

Urban studied, therefore, the effect of progressive practice in lifted weight experiments during an extended series of judgments. We believed that it was of advantage to obtain a clearer picture of the improvement during the initial stages of progressive practice.

Our materials and methods were similar to those employed by Urban. Small brass cylinders were used as stimuli, which were arranged in pairs. Five such pairs were employed, each consisting of a standard stimulus of 100 grams and a comparison stimulus of 88, 92, 96, 100 or 104 grams. These stimuli were placed about the circumference of a table with a revolving top so that the stimuli could be brought successively directly under the hand of the subject. Thus the space errors were eliminated. The time errors were present in the first order, *i. e.*, the standard stimulus was always lifted first. The rate of lifting was regulated by the beats of a metronome so that the time-error remained constant. Immediately after the lifting of each comparison weight a judgment was given verbally in terms of the second weight, the subject employing the three categories of lighter, equal and heavier, which were defined in the usual manner. The observed relative frequencies of the different categories upon each of our comparison pairs thus obtained were treated in accordance with the methods of calculation developed by Urban.[3]

With these materials and with this manner of presentation, we obtained 100 judgments upon each of the 5 comparison pairs from each of ten naive subjects.[4] The experiments were divided into two sessions of one hour each. At the beginning of the first session we explained the manner of lifting and then gave the subject practice in making the hand-movements in time to the metronome and in grasping and lifting a single weight. Not until these hand-movements had become reasonably regular did we begin our series; and all of the judgments given after the first round or two of the table were recorded. After this preliminary instruction and practice in the hand-movements, we succeeded in obtaining 50 judgments on each of our comparison pairs from the subject during the first session. At a later session, after an interval of at least twenty-four hours, we obtained 50 more judgments from the subject upon each of our comparison pairs. Frequent rest-periods were introduced in order to eliminate the fatigue factor.

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[3] For a more detailed description of the stimuli, the method of lifting and the treatment of results, *cf.* Fernberger, *ibid*, 6-11; S. W. Fernberger, On the Elimination of the Two Extreme Intensities of the Comparison Stimuli in the Method of Constant Stimuli, *Psychol. Rev.*, XXI, 1914, 337-340; F. M. Urban, Hilfstabellen für die Konstanzmethode, *Arch. f. d. ges. Psychol.*, XXIV, 1912, 236-243.

For purposes of comparison, we have divided these hundred judgments on each pair into four groups of 25 judgments each in the order in which they were taken. Hence Series 1 comprises the values of the first 25 judgments on each comparison pair; Series 2 the second 25; Series 3 the third 25; and Series 4 the fourth 25 judgments. The time of experimentation was relatively short, but the calculations involved were rather long and involved the calculation, in all, of 120 different threshold values.

The results of these calculations for the different subjects are found in Tables I-X, each table being given to the results obtained from a single subject. Table XI gives the averages of the results of all ten subjects. All of the tables show a similar arrangement. The first column contains the numbers of the series, *i. e.*, the groups of 25 judgments upon each comparison stimulus in the order in which they were taken. The second and third columns contain the values of h_1 and h_2, the coefficients of precision of the curves of the psychometric functions for the lighter and heavier judgments respectively. The next two columns labelled *D* and *I* give the values of the lower and upper thresholds respectively. The next column gives the value of the interval of uncertainty (*I-D*). It will be remembered that one-half of this interval is the Threshold of Volkmann, which is recognized as the measure of sensitivity of the subject. Finally, the last column contains the values of the point of subjective equality. In each row will be found the values obtained from a treatment of the relative frequencies of the series indicated in the first column.

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[4] My thanks are due to the following, who kindly consented to act as subjects in this investigation: Misses E. Gough and M. L. Grimes, both Assistants in Psychology at Wellesley College; Mr. F. J. O'Brien, Assistant in Experimental Psychology at Clark University; Misses E. Bowman and F. Mateer; and Messrs. H. R. Crosland, B. Hori, H. H. Long, R. B. Teachout, and R [illegible] [illegible]nced students in psychology at Clark Uni[illegible] to Professor J. W. Baird for his [illegible] of the manuscript.

TABLE I

Series	h_1	h_2	D	I	*Interval of uncertainty*	*Point of subjective equality*
1	0.09796	0.12864	94.75	98.14	3.39	96.44
2	0.07414	0.07522	95.96	98.02	2.06	96.99
3	0.10454	0.09420	93.13	97.70	4.57	95.42
4	0.12952	0.13644	92.74	95.63	2.89	94.18

TABLE II

Series	h_1	h_2	D	I	*Interval of uncertainty*	*Point of subjective equality*
1	0.10777	0.10272	90.62	98.33	7.71	94.48
2	0.09352	0.12143	92.90	97.27	4.37	95.08
3	0.09582	0.11228	91.53	98.44	6.91	94.98
4	0.11875	0.10686	92.56	98.05	5.49	95.30

TABLE III

Series	h_1	h_2	D	I	*Interval of uncertainty*	*Point of subjective equality*
1	0.07063	0.08034	92.81	99.10	6.29	95.96
2	0.09471	0.07772	94.93	99.86	4.93	97.40
3	0.08161	0.09972	93.74	97.62	3.88	95.68
4	0.10392	0.09989	94.67	98.41	3.74	96.54

TABLE IV

Series	h_1	h_2	D	I	*Interval of uncertainty*	*Point of subjective equality*
1	0.12776	0.13971	96.86	99.79	2.93	98.32
2	0.12348	0.15051	98.64	100.69	2.05	99.66
3	0.14540	0.12635	96.30	99.10	2.80	97.70
4	0.12479	0.13280	96.22	98.16	1.94	97.19

TABLE V

Series	h_1	h_2	D	I	*Interval of uncertainty*	*Point of subjective equality*
1	0.04786	0.08470	90.37	102.18	11.81	96.28
2	0.10187	0.11359	91.02	99.62	8.60	95.32
3	0.12352	0.12159	91.90	100.30	8.40	96.10
4	0.11625	0.09344	91.62	100.96	9.34	96.29

TABLE VI

Series	h_1	h_2	D	I	*Interval of uncertainty*	*Point of subjective equality*
1	0.12524	0.11658	93.64	96.26	2.62	94.95
2	0.10684	0.12601	93.18	96.64	3.46	94.91
3	0.15475	0.10886	92.41	96.31	3.90	94.36
4	0.14100	0.11280	93.32	95.66	2.34	94.49

TABLE VII

Series	h_1	h_2	D	I	*Interval of uncertainty*	*Point of subjective equality*
1	0.07108	0.07576	91.93	99.16	7.23	95.54
2	0.07554	0.09222	92.17	97.37	5.20	94.77
3	0.08500	0.08015	92.13	98.98	6.85	95.56
4	0.09400	0.08321	92.17	98.26	6.09	95.22

TABLE VIII

Series	h_1	h_2	D	I	*Interval of uncertainty*	*Point of subjective equality*
1	0.09365	0.11546	92.69	105.56	12.87	99.12
2	0.13934	0.09797	90.85	104.55	13.70	97.70
3	0.11675	0.12700	90.86	102.52	11.66	96.69
4	0.08441	0.10144	90.67	101.99	11.32	96.33

TABLE IX

Series	h_1	h_2	D	I	*Interval of uncertainty*	*Point of subjective equality*
1	0.09038	0.08279	88.13	100.43	12.32	94.29
2	0.06068	0.12156	90.84	98.76	7.92	94.80
3	0.07990	0.09621	91.71	99.01	7.30	95.36
4	0.11394	0.14412	92.76	98.48	5.72	95.62

TABLE X

Series	h_1	h_2	D	I	*Interval of uncertainty*	*Point of subjective equality*
1	0.05983	0.07900	93.22	105.08	11.86	99.15
2	0.10953	0.09884	94.49	101.79	7.30	98.14
3	0.11907	0.09212	94.36	101.67	7.31	98.02
4	0.12128	0.08246	93.83	99.99	3.08	96.91

TABLE XI

Series	h_1	h_2	D	I	*Interval of uncertainty*	*Point of subjective equality*
1	0.08922	0.10058	92.50	100.40	7.90	96.45
2	0.09796	0.10751	93.50	99.46	5.96	96.48
3	0.11064	0.10585	92.81	99.16	6.36	95.99
4	0.11479	0.10935	93.06	98.56	5.50	95.81

An examination of the values contained in Table XI indicates that the same tendencies are present for our series as were found by Urban, only these tendencies are even more marked. The values of h_1 (the coefficient of precision for the lighter judgments) show a steady rise and one which is more marked for the early series than for the later. The values of h_2 (the coefficient of precision of the heavier judgments) show a marked tendency to increase especially for the earlier series. The curve does not rise steadily, however, inasmuch as the value for the third series is slightly smaller than that for the second. The values of the threshold in the direction of decrease (D) remain fairly constant. They show variations of an unsystematic character such as one usually finds in lifted weight experiments, but no systematic tendency is evident. The values of the threshold in the direction of increase (I), on the other hand, show a marked tendency to decrease which again is most marked between the first and second series. The values of the interval of uncertainty show a marked tendency to decrease, particularly marked at first. This decrease is broken at one point; the value for the third series being slightly larger than that for the second series. The values for the point of subjective equality similarly show a tendency to decrease, but not so markedly as for some of the other values. This tendency is broken at one point; the value for the second series being very slightly larger than that for the first.

Our results, then, are precisely similar to those obtained by Urban, except that the tendencies indicated are here more marked. This we would expect, since we are dealing with the initial stages of progressive practice, while Urban was dealing with the effects of progressive practice for an extended series of experiments. In our results the course of the general tendencies to increase or decrease are broken in only three places; the value of h_2 for the third series, the value of the interval of uncertainty for the third series, and the value of the point of subjective equality for the second series. Regarding this last value we can only say that the effect of the initial practice is about equally strong upon both thresholds; that at the first the lower threshold tends to assume a slightly greater value to about the same exte
the value of the upper threshold is decreased. Henc
two thresholds approach each other in approxin
same amount, the point of subjective equality rer
cally unchanged.

The other two apparently anomalous valu

ily explained, and indeed, strengthen our belief that progressive practice is the sole factor which is operating here. Progressive practice is essentially a form of learning process. In the case of any learning process there is always something of a loss, due to forgetting during a rest-period, so that in starting out after the rest the subject does not have as high a degree of efficiency as he had at the end of the work-period just before. It will be remembered that we obtained the first two groups of 25 judgments on each comparison pair during the first day's experimentation for each subject, and the last two groups during a second experimental period. Hence there was a rest-period of more than one day between the second and third groups. During this period some of the practice obtained during the first period, we may well believe, was lost, so that, for the third group the subjects started at a lower degree of efficiency than they had had at the ending of the second group. This, indeed, is reflected throughout the results and shows particularly in the two apparently anomalous values of the h_2 and the interval of uncertainty of the third series, which are slightly lower and higher, respectively, than the corresponding values of the second group.

Although we have this exceedingly great regularity of the different tendencies of increase and decrease for the different values under consideration for the averages of all ten subjects, still the values for the individual subjects do not show quite such great regularity. Nevertheless the values for every subject show more or less markedly the same general tendencies of increase and decrease of the different values. The values for one subject (Table X) show practically a regular increase and decrease in values with one exception; the values of the interval of uncertainty for the second and third groups are practically identical. One would not expect great regularity in the values of the individual subjects, because relative frequencies from 25 judgments on each comparison pair are far too small a number to be considered ideally as probabilities. Still we were forced to use such a small number for purposes of comparison if we wished to study the initial effects of progressive practice. We believe that much greater reliance is to be placed upon the averages for all of the subjects as in this case accidental variations would tend to cancel one another.

The fact that progressive practice in its initial stages has such a marked effect upon the position of the thresholds and upon the values of the interval of uncertainty and the point of subjective equality is of exceedingly great significance in the application of the psychophysical methods for purposes of anthropometric measurements. Anthropometrists seek to determine the sensitivity of the subjects or the point of subjective equality with as few a number of judgments and in as short a time as possible. The method of constant stimuli recommends itself for such measurements inasmuch as it is by this method alone that the subject can be kept in entire ignorance of the objective relations of the stimuli. The calculations involved by the use of the method of constant stimuli are rather lengthy unless the results are taken in such a form that one may apply Urban's tables, referred to above. One of the conditions for the application of these tables is that the number of judgments obtained must be exactly divisible into 100. Hence we may employ the tables only in those cases where we have obtained either 10, 25, 50 or 100 judgments upon each comparison pair. In order to show the effect of this initial practice upon the number of determinations obtained, we have calculated the intervals of uncertainty and the points of subjective equality for each of our ten subjects for the first 25, the first 50, and the first 100 judgments on each of our comparison pairs. The first values obviously correspond to the first rows of Tables I-X. The second values are calculated from the observed relative frequencies of groups 1 and 2 of the former tables. The last values are calculated from the relative frequencies of all of the determinations made, namely, groups 1, 2, 3 and 4.

TABLE XII

Subjects	*Interval of uncertainty*			*Point of subjective equality*		
	25	50	100	25	50	100
A	3.39	2.84	3.50	96.44	96.31	95.61
B	7.71	6.66	6.35	94.48	94.81	95.02
C	6.29	5.57	4.76	95.96	96.70	96.35
D	2.93	2.27	2.48	98.32	98.86	98.35
E	11.81	9.60	9.04	96.28	96.02	96.19
F	2.62	2.69	3.12	94.95	95.10	94.76
G	7.23	6.08	6.47	95.54	95.17	94.70
H	12.87	13.81	12.96	99.12	98.40	97.20
I	12.32	10.19	8.31	94.29	94.60	95.18
J	11.86	9.23	7.87	99.15	98.62	98.02
Average	7.90	6.89	6.49	96.45	96.46	96.14

The values obtained from this calculation are given in Table XII. The first column contains the letters of the subjects. The next three columns contain the values of the interval of uncertainty,—the first of these, those for the first 25 judgments on each comparison pair, the next for the group of 50 judgments and the third for the group of 100 judgments. The next three columns show a similar arrangement as regards the groups and contain the values of the point of subjective equality. The last row of values in this table contains the averages for each column. If we consider the averages for the point of subjective equality, we find that the values for the group of 25 judgments is almost identical to that for the group of 50 judgments. Both vary slightly from the value for the entire group of 100 judgments. But the variations are so slight that we may consider them to be negligible and dismiss the point of subjective equality from our discussion.

A very different state of affairs is to be found when we compare the averages for all of our subjects of the intervals of uncertainty for the different groups. If we compare the group of 100 judgments with the group of the first 50 judgments, we find that the value for the latter is somewhat larger, the difference being just 0.4 grams. The difference between the values for the group of 100 judgments and the group of the first 25 judgments is considerably larger, being 1.41 grams, or over three and a half times as large as the difference between the values of the 100 and 50 judgment groups.

Now, in anthropometric measurements we have two ideals to which we must more or less closely conform—time and accuracy. We cannot actually conform to the ideal of accuracy because it was shown by our results and those of Urban that this factor of progressive practice may be observed up to the 4th-6th group of 100 judgments. In making anthropometric measurements one cannot take so many reactions from a single individual as this would outrage the principle of the time required to obtain the judgments.

On the other hand, the small number of determinations advocated by certain writers is obviously too small. Whipple,[5] for instance, advocates the taking of only 10 determinations; but in the light of our findings such a procedure may furnish an exceedingly uncertain and inaccurate measurement of the sensitivity of the individual. If the interval of uncertainty is so much larger for a group of 25 judgments upon each comparison pair than that for 100 judgments, we must certainly conclude that any procedure which employs less than 25 judgments is mischievous and inadmissible.

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[5] G. M. Whipple, *Manual of Mental and Physical Tests.* Baltimore, 1910, 188-193.

The anthropometrist finds himself between Scylla and Charybdis; if he makes a large number of determinations he sacrifices more time than is ordinarily at his disposal, and if he makes a small number of determinations his results are inaccurate and valueless. And in his extremity he may very pertinently inquire: What is the least number of determinations upon which the measurement of an individual's sensitivity may safely be based? In the light of our findings it would appear that 50 determinations upon each comparison pair is the smallest number upon which an accurate measurement can be based. We have also found that an average subject may be taught the technique of lifting and also that we may obtain 50 judgments on each comparison pair by our methods within a single session of an hour's duration. This includes frequent and sufficient rest-periods. This arrangement, furthermore, has the advantage in that the subject is not kept too long at the task; and on the other hand, may be disposed of in a single session. We believe that an hour for the determination of the sensitivity of each subject is not too much to ask from either the anthropometrist or the clinician. Indeed, our results show that he must spend this time if he is going to employ lifted weight experiments to obtain results which will have an admissible degree of accuracy.

Obviously it is well to obtain as many judgments as possible, because the greater the number of judgments the greater will be the accuracy of the determinations. But in anthropometric measurements, the time required for the obtaining of the results is an important factor, and hence one is impelled to set arbitrary limits as a sort of compromise between accuracy and time. We suggest, on the basis of our results, that 50 judgments upon each of 5 comparison pairs is a sufficient number to require from each subject for the ordinary anthropometric study. Our results show that the values obtained do not vary greatly from those obtained from a group of the first 100 judgments. The first 50 judgments upon each pair of stimuli may be obtained within the space of one hour's sitting, including the preliminary practice in the technique of lifting. We do insist that no determination of less than 50 judgments upon each comparison pair is admissible inasmuch as the results obtained violate the principle of accuracy. The divergences in the results are due to the effect of the progressive practice obtained by the subject during the [illegible]i-mentation. The effect of this progressive practice is to increase the values of the coefficients of precision and to decrease the size of the values of both the interval of uncertainty and to a less degree of the point of subjective equality. The effects of this progressive practice are stronger at the beginning of the experimental series and decrease at first rapidly and then more slowly as the experimentation continues.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. Titchener and H. P. Weld

XXVII. Simplicity vs. Complexity of Color Hues

By E. M. Alspach

In the experiments here reported, we have sought to obtain introspective evidence with regard to the alleged compositeness of color hues. The opinions of psychologists and color-theorists concerning the existence of composite colors may roughly be classified into three groups. (1) All colors, with the exception of black, white, red, yellow and blue are composite; orange, for example, is composed of red and yellow; green is composed of blue and yellow. (2) In addition to the simple colors of the first group, green is also simple. It is true that green may be obtained by mixing blue and yellow pigments; but still it is a quality which is neither blue nor yellow; and it cannot, therefore, be factored into blue and yellow. All other colors, however, are composite. (3) No colors are composite; all colors are psychologically simple. The psychological orange may be obtained by a psychophysical mixture of red and yellow, but the resultant quality is simple; however *like* red and yellow it may appear, it cannot be analysed into these two components. Bentley has shown that this disagreement is partly a result of different methods of analysis, partly of a confusion of the psychological and the psychophysical points of view.[1] In this paper we shall avoid, so far as possible, all theoretical considerations: if our problem has any relation to color-theory, it is indirect; and all arguments from qualitative opposition, color-mixture, contrast, color-blindness, etc., are beside the point. We are concerned solely with psychological (not logical, psychophysical, physiological, physical, or technical) analysis of color hues.

It has been pointed out above that those who believe in the existence of composite hues do not agree among themselves, and that their disagreement turns on the character of green. The principal advocate of composite green is Brentano.[2] He approaches the problem phenomenologically; he thinks that he can see B and Y in green; and he calls to witness Goethe and many other persons, especially painters, who are assumed to be especially skilful in the analysis of colors. With his position Holt[3] is in agreement. This view means that if all the hues between Y and B are arranged in a series, they fall on a single straight line, and G lies about the middle; just as O lies in the middle of the R-Y series. Katz[4] argues against the position on the ground that the two series, when directly compared, are not in all respects similar; a hue, for example, which lies in the B-Y series near the Y appears greenish, not bluish; while a hue occupying a similar position in the R-Y series appears not orangeish but reddish. Furthermore, it is not true that painters are more skilful in the psychological analysis of color than are psychologists themselves. The two professions have different points of view; when the painter analyses a color, his problem is to determine what pigments will reproduce that color on his palette; he, therefore, 'sees' in a given hue the colors of its pigment-components; while the psychologist sees hues which are similar to other hues.

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[1] M. Bentley, The Simplicity of Color Tones, *Amer. Jour. Psychol.*, xiv, 1903, 92 f.

[2] F. Brentano, *Untersuchungen z. Sinnespsychologie*, 1907, 1 ff; 129 ff.

[3] E. B. Holt, The Place of Illusory Experience [illegible] Realistic World, in *The New Realism*, 1912, 333 ff.

[4] D. Katz, *Die Erscheinungsweisen der Farben*, 1911

The painter, now, would willingly accept the challenge to reproduce the seen color from the 'seen' components. Brentano also, if we take him at his word, would accept this challenge. "Die Aehnlichkeit, die Orange einerseits mit Rot und anderseits mit Gelb hat, ist nicht derjenigen zu vergleichen, die etwa einem Ton zwischen *c* und *e*, z.B. dem zwischen ihnen gelegenen *d*, mit jenem tieferen und diesem höheren Ton zugeschrieben werden kann, sondern offenbar derjenigen, welche der Zweiklang *ce* mit den beiden Komponenten zeigt. Man erkennt darin die beiden Farben, wie man dort die beiden Töne heraushört."[5] Composite colors are fusions, then, similar to tonal fusions, and may be analysed in the same way. We may suppose, therefore, that as in the analysis of the tonal fusion *ce* we come out with two tones, which may be named by their vibration frequencies, and which when produced together will reproduce the original fusion, so in the analysis of a color fusion we should be able to name or point out the components, and thus to reproduce the original fusion. The obvious test is recourse to experiment.

We know of only two attempts to attack the problem experimentally. Katz[6] reports an experiment with 80 school-girls, 8-9 years of age, who presumably had no knowledge of color-mixing, but who named the colors correctly. They were unable to analyse successfully either orange or green, although they had been shown how a violet could be obtained by mixture, and although in most cases they were clever enough to avoid choosing the same pairs of components in both cases. Mrs. Ladd Franklin,[7] who has long contended that R, Y, G and B are 'psychically unitary,' and that the intermediate colors are non-unitary colors or 'color-blends,' has devised an experiment, the results of which, she thinks, proves her position. She has not published her results in detail, however, and we have deemed it advisable to repeat her work. Her method will be discussed, later, in connection with our own results.

Those psychologists who believe in the simplicity of color hues contend that what their opponents 'see' in colors is only a *likeness* to other colors; that O, for example, is *like* R and *like* Y, not that O *is* R and Y. Bentley long since pointed out that the colors about the base of the color pyramid fall into two major groups; "the one contains the reds and yellows, the other the greens and blues. Within each group there is a special kinship which marks off one group from another."[8] Katz, who is very cautious as regards the composite nature of O and V, remarks that in his experience G has a similarity to the colors Y and B which is greater than that existing between G and R. In Westphal's experiments, where among others the best G (*Urgrün*) was determined, Katz reports as observer that the same stimulus appeared *successively* bluish and yellowish; but, he adds, "die spurenweise Andeutung der Nachbarfarben in den Urfarben, wie sie bei dieser Einstellung vorhanden ist, ist natürlich von ganz anderer Art als das Enthaltensein je zweier Urfarben in den zwischen ihnen liegenden Farbentönen, z.B. des Rot und Gelb in dem Orange."[9] What this difference is he does not say; he thinks that the brightness and the feeling-tone of G lie midway between those of B and Y (the same thing might be said also of R); but he does not commit himself with regard to the composite character of O and Y. Westphal found that, in the determination of *Urfarben* (e. g., *Urrot*), it is not always possible for the observer to fix the point where the hue concerned does not evince a close relation to the neighboring hues (*weder gelblich noch bläulich erscheint*). If, he continues, Brentano concludes that the sensation green is a sensation yellow-blue, because some persons can see Y and B in it, then with equal right one may conclude that the sensation R is yellow-blue, and the sensation Y is red-green.[10]

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[5] *Op. cit.*, 16.

[6] *Op. cit.*, 366 f.

[7] C. L. Franklin, Determination of the Psychically Unitary Color-Sensations, Report of the Philadelphia Meeting of the Amer. Psychol. Asoc., *Psy. Bull.*, xii, 1915, 62 f.

[8] *Op. cit.*, 92 f. H. E. Houston and W. W. Washburn *Amer. Jour. Psy.*, xviii., 1907, 523) find no overlap of the names B and Y, and argue from this fact that the two colors cannot be visible in G.

In view of this divergence of opinion, we (1) planned an experiment which we hope is without prejudice, and which is purely analytical in its nature; (2) we have repeated Mrs. Ladd Franklin's experiment; and (3) we have performed a variant of Westphal's experiment. The procedure of these three experiments, together with their results, will be taken up in order.

EXPERIMENT I

The object of this experiment was to test the observer's ability to analyse colors, and to compare the results with those of observers who selected colors which were 'like' the stimulus. We employed as stimuli the following colored papers, all of the best available chroma and medium tint: R, O, YO, Y, YG, G, BG, B, V, P; and, in addition, both light and dark tints of O, G, V, and a medium tint but poor chroma of O, G, B and V. The O was taken from the Milton Bradley Co.'s Pure Spectrum Scales; all the other stimuli were Hering papers. The tints and lesser chromas were obtained by mixing in the requisite amounts of Bk-W.

The observer sat at a table facing a screen. The screen had a square window, which could be closed by a shutter. Immediately behind the screen stood a color-mixer, upon which the various stimuli were shown. On the table before the observer was a tray of sample colors, 3 x 6.5 cm., mounted on grey cardboards of slightly larger size. These color samples, 130 in number, were representative of the entire color circle with varying hues, tints and chromas, and also included a few greys.

When the stimulus was presented, the observer was asked the following question: "Does it appear simple (homogeneous), or do you see other colors in it?" In case he reported 'simple,' he was asked: "Is it like any other color?" If the answer was 'yes,' he was then instructed: "Choose (from the samples) the colors which it is like." But if the answer was 'no,' he was asked to name the presented color, and the experiment came to an end. If, however, in answer to the first question, the observer found 'other colors' in the presented color, he was then instructed: "Choose (from the samples) the colors that you see." When this had been done, he was asked to name the colors he had chosen, and also to name the stimulus color.

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[9] *Op. cit.*, 363.

[10] H. Westphal, Unmittelbare Bestimmungen der Urfarben *Sinnesphysiol.*, xliv, 1909-10, 198, 230 n.

After the experiments had progressed for some time it seemed advisable to reduce the number of stimuli by striking out the YO, the P, the light and dark tints, and the lesser chromas, since these colors did not yield results sensibly different from the others. In the experiments which we shall report, therefore, we employed eight stimuli, all of the best possible obtainable chroma. We had twenty-three (in some cases twenty-four) observers. About half of the observers had had some training in psychological observation; some were instructors in psychology, others were students in the psychological laboratory; of the other half, four were girls, 12-15 years of age, who had had some experience with mixing pigments, and the remainder were college students taken at random. We give the results in Table I. In preparing the table we have grouped the colors which were chosen as 'likes' or as 'components' into 19 classes, represented by the color symbols at the head of the vertical columns; e. g., the column R contains all the hues which were distinctly red (neither bluish nor yellowish), but which differed in chroma, tint, texture, etc. We have classified the observers into trained (Tr) and untrained (Un), and also according to the nature of their judgments (simple or composite). The Table, then, shows the distribution of the colors chosen by each of the two classes of observers, and according to the two types of judgment. It should be remarked that, in the instruction given, there was no limit to the number of colors which an observer might choose as 'like' the stimulus or which he might 'see in' the stimulus; the sum of the numbers on a horizontal line is therefore often greater than twice the number of observers.

There are three gross results. (1) The judgments of likeness and of composition of color hues can be made, and are made, by all observers, trained and untrained. But, in general, the choice of components shows practically the same distribution as the choice of likes; so that the basis of judgment in the two cases is perhaps similar, if not identical. (2) Only in exceptional cases are R, Y, G, and B reported as components of (or as like) the stimulus color. The principal exception is that, when one of these colors is itself the stimulus, it is most 'like' itself, or is 'composed of' itself. In the case of intermediate colors, O, YG, BG, and V, the tendency of distribution is *not to reach* the corners of the color square. (3) There is evidence, in judgments both of simplicity and of complexity, that the observer tends to restrict his choices to a single side of the color square. Thus in the case of intermediates the distribution is approximately symmetrical, whereas in that of the corner colors the tendency is for the judgments to occur only on the one side; this latter tendency is greater for R and Y than for G and B.

It seems to us that, in the face of these results, but one conclusion is possible; that, whatever some of our observers saw in the stimulus, they did not see component colors. So far as this experiment goes, our results are totally at variance with the view that the intermediate colors are fusions or blends, comparable with tonal fusions. Further-

TABLE I

Stim.	No. of Obs.	Judgment	VR	R	OR	RO	O	YO	OY	Y	GY	YG	G	BG	GB	B	VB	BV	V	RV	Bk-W
R	7 Tr	simple		6	3	1		1	2												
	7 Un	"		6	1	1				1											
	4 Tr	composite		2	2	1		1	1												2
	5 Un	"		3	1			1	1	1								1			2
O	9 Tr	simple			4	1	8		4	1											
	6 Un	"				1	6	1													
	3 Tr	composite			1	2	1		2												
	6 Un	"		1	2	1	3	1	1	2	1										
Y	6 Tr	simple							1	7	3	1									
	9 Un	"							1	6	1	3									
	5 Tr	composite								6	4	3									
	3 Un	"								3	1					1					
YG	6 Tr	simple								2	1	7	1								
	4 Un	"									3	4									
	5 Tr	composite								4	1	5	3								
	8 Un	"								4	4	3	3			1					
G	10 Tr	simple								2	1	3	9	2					2		
	4 Un	"										1	3								
	1 Tr	composite											1								2
	8 Un	"								3	3		7	1							1
BG	6 Tr	simple									1		1	6	1						
	5 Un	"												7	1				1		
	6 Tr	composite										2	2	3	2	1	1				3
	7 Un	"								1			1	5	4	1					
B	11 Tr	simple										2	2			11	2		1		
	10 Un	"												1	1	11	1		1		
	1 Tr	composite														1					1
	2 Un	"	1													2					
V	7 Tr	simple	1	3					1							1	2	1	5	1	
	6 Un	"	1														3		5	2	
	5 Tr	composite	2	1												1	1		3		2
	6 Un	"	2	1	1												2	1	4		

more, as regards the question of the composite character of green, we point out: (1) that the distribution of choices, whether of similars or of components, conforms to the type of distribution for the other corner colors; (2) that in no case is a blue or even a greenish blue chosen as a component of G; and (3) that those who 'saw other colors' in the stimulus most frequently chose, as components, colors which belonged to the same group as the stimulus. These facts all tell against the composite character of green.

Finally, we remark that, on the basis of similarity, or because some observers can see other colors in them, the corner colors themselves, B alone excepted, might be regarded as composite. It is true that we employed colored papers, which are notoriously impure; but then pigments are also impure; and our results are in accord with those of Westphal, who employed spectral lights as stimuli.

EXPERIMENT II

We thought it well, at this point, to repeat Mrs. Ladd Franklin's experiment. The only published account of her method and results is as follows. "By exhibiting at once (say) seven colors, on discs rotated by a common electric motor, made practically equal in intensity and saturation but just noticeably different in color tone, it is found that all observers can distinguish between the unitary colors and the color blends. The judgment is a perfectly easy one to make; none of our observers failed to make it save one, and he turned out to have dichromatic vision—which involves of course the impossibility of ever seeing a color blend."[11] We have supplemented this meagre account of method by written notes taken from Mrs. Ladd Franklin's verbal report at the Philadelphia meeting. The experiment as we performed it was as follows. In a first series, we set up seven compound discs which, when rotated, gave a series of colors practically equal in tint and chroma but just noticeably different in hue, passing from a bluish red through red to a yellowish red. In a second series, the hues ranged from a reddish blue through purple to a bluish red; and in a third series, from a yellowish red through orange to a reddish yellow. The instruction for the first series was: "Pick out a color which is neither bluish nor yellowish. Which colors are bluish? which colors are yellowish?" The instruction for the second series was the same, except that 'reddish' was substituted for 'yellowish.' When the results from this instruction (which was Mrs. Ladd Franklin's) had been obtained, we supplemented the instruction by asking: "Which colors are unitary? Which are complex?" In the third series we further modified the instruction by asking the five questions: "Which colors are yellowish, which are orangeish? Which are reddish? Which are complex? Which are unitary?" There were 16 observers, four of whom were trained, six were taking their first course in the laboratory, and the remaining six had had no experience in psychological observation. The results appear in the following Tables, which show the distribution of the judgments for each one of the seven discs.

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[11] *Op. cit.*, 63.

TABLE II

SERIES 1

	BR—	—	—	R	—	—	—RY
Number of disc......	1	2	3	4	5	6	7
Bluish..............	16	16	8	......	......	......	......
Neither B nor Y.....	......	......	8	13	2	......	......
Yellowish...........	......	......	......	4	15	16	16
Complex............	10	11	8	6	8	12	11
Unitary.............	6	5	8	10	7	4	5

SERIES 2

	BR—	—	—	P	—	—	—RB
Number of disc......	1	2	3	4	5	6	7
Bluish..............	15	16	15	10	8	7	6
Neither B nor R.....	1	......	1	5	2	......	1
Reddish.............	3	4	6	9	13	15	14
Complex............	4	8	8	10	10	10	6
Unitary.............	12	8	8	6	6	6	10

SERIES 3

	YR—	—	—	O	—	—	—RY
Number of disc......	1	2	3	4	5	6	7
Yellowish...........	6	5	6	8	13	16	16
Orangeish...........	5	7	9	13	14	7	4
Reddish.............	16	15	14	13	8	3	2
Complex............	7	9	8	8	10	9	4
Unitary.............	9	7	8	8	6	7	12

These results, we suppose, would have been satisfactory to Mrs. Ladd Franklin,—had we not asked for judgments of unitariness and complexity! As they are, they do not bear out her conclusion that "all observers can distinguish between the unitary colors and the color blends." Of the 16 observers, 6 regard red as complex, and 5 find neither red nor blue in purple. A still more curious result is that for O in the 3rd series: 13 see it as reddish, 13 as orangeish, but only 8 see yellow in it, and only 8 see it as complex; evidently, some persons do not see yellow in orange, and some see orange as unitary. Such examples might be multiplied; but it is obvious that, although the form of the curves for bluish, neither bluish nor reddish, and reddish might be satisfactory to Mrs. Ladd Franklin, the fact that *on the average only one-half of the observers regard the colors in the R-B series and in the R-Y series as complex* d
square with her logical conclusion that resemblance impl
character, and that since purple (for example) resem
and B, it must therefore be psychologically a color

of this confusion of the logical and the psychological methods of analysis, the method of this experiment is inadequate to the problem; and for the same reason the results are at variance with those of our first experiment.

EXPERIMENT III

In this experiment we attacked the problem of the compositeness of green by a metric method. We prepared discs of blue-green and yellow-green, which were not so far removed in the color series but that, by the second law of color mixture, their mixture gave a green of good chroma. With a preponderance of the BG the mixture was decidedly bluish; and with an excess of YG the mixture was distinctly yellowish. The problem was to determine the points of change on the one hand between blue and not-blue, and on the other hand between yellow and not-yellow. The method employed was that of Constant Stimuli. Five stimuli were selected, with intervals of 3°. These were arranged by hazard into 100 series, so that 500 observations were made under each instruction. In the first experiment the instruction was: "You are to judge Bluish or Not-bluish." The observer was told, emphatically, that 'not-bluish' did *not* mean yellowish or greenish or pinkish; it meant not-bluish and nothing more; he was also urged to make every effort to keep his attitude constant throughout the experiment. In the second experiment the categories were 'yellowish' and 'not-yellowish,' and the observer was further instructed as in the first experiment. We hoped, by thus formulating the instructions, to accomplish two things: (1) to eliminate the question of green entirely (it will be apparent that only by implication can 'green' come into our final results), and (2) to discover whether or not it is possible, under experimental conditions, to carry a single attitude ('bluish' or 'yellowish') through the Blue-Yellow series. We believe that on both counts we have weighted the experiment, if at all, in favor of Brentano's contention. The observers were Dr. L. D. Boring (L. D. B.), Dr. W. S. Foster (F), Dr. E. G. Boring (B), and Miss M. E. Wright (W), a student in quantitative psychology. The first three observers have had long experience in psychological observation. Only the last two, however, were able to complete the entire series; and although the others completed the preliminary series and proceeded far enough in the experiment proper to give an indication of the final outcome, we shall not consider their quantitative results. In the Table which follows the value of RL equals degrees of YG.

TABLE III

		RL	h	Interval btwn. Y and B
Obs. B.	Bluish or not........	87.868	.1669	12.627
	Yellowish or not.....	75.241	.1640	
Obs. W.	Bluish or not........	44.868	.1092	17.024
	Yellowish or not.....	61.492	.1315	

These figures mean that, for both observers, the blue-yellow series is neither blue nor yellow throughout its course: there is a point (in the sense of a limen) where blue changes to not-blue, and another point where yellow changes to not-yellow. There is a difference, however, between the two observers. For B. the blue and yellow areas cross each other; or, to put it in another way, there are in the B-Y series (1) an area that is bluish but not yellowish, (2) an area that is *both bluish and yellowish,* and (3) an area that is yellowish but not bluish. For W., on the other hand, the two extreme areas do not meet; there is an intermediate area that is *neither bluish nor yellowish.* It is clear that either result is fatal, so far as this experiment goes, to Brentano's notion that all colors between blue and yellow are components of these two extremes.

While, as has been said, the other two observers did not complete their series, the results obtained agree in principle with those of W. The intermediate interval (neither bluish nor yellowish) for F. was about 21°, for L. D. B. about 16°. The introspections also show that these three observers had difficulty in carrying their instruction through the region of green. W. says: "About the middle of the series, judgments of yellowish and bluish are difficult. . . . Green is so pronounced that it is hard to say whether the color is yellowish or bluish." L. D. B. reports: "I think of the color series from Y to B through G as a straight line, and I attempt to place the stimulus in position with reference to that series. . . . There are times when, disregarding my *Aufgabe,* I asked myself 'Is it yellowish or bluish green or just a good green?'" F. also reports: "Sometimes visual images of the spectrum come up, particularly the green and yellow parts; green is seen in such cases. The chief processes are auditory kinaesthetic words; but besides these visual image of true green, and (rarer) a visual image of yellow." B., on the other hand, was frequently influenced in his judgment by secondary criteria, and perhaps on this account was better able to disregard the insistency of green. He says: "The judgment 'yes' (Yellowish) is a positive reaction on my part to the stimulus; the yellowish hue is somewhat alive, keen, sharp, bright, not smooth, not uniform; early in the experiment I wondered whether yellowish might not be a slight painful complex in the eyes. The judgment 'no' (Not-yellowish) is an equally positive passive acceptance. The presentation is smooth, comfortable, quiet, calm; I don't know how much of this is my reaction, and how much is immediately inherent in the color. . . . The blue was soft and comfortable as against the blare of the yellow; all a matter of eye pressures and pains."

The color experience in these experiments did not always appear simple. Three of the observers occasionally reported a 'haze' over the color; for W. and F. this was yellowish, and influenced their judgments. B., who has given the most complete account, says: "Sometimes I think I see spots that are more bluish than others or perhaps are even blue; sometimes these spots give the impression of seeing the disc through a bluish haze; sometimes the spots or haze seem to come and go as if due to a fluctuation of attention or determination. The vagueness of this haze makes me wonder whether it is a haze at all, or whether it is not a shifting underdetermination to a tied image, like a memory-color." "In general the 'yes' hue was complex, whereas the 'no' hue was simple. I mean that in taking the hue as yellowish I never took it as yellow, but always as if yellow were a part of it. The 'not-yellow' was always just something that belonged all together, seldom green or blue, but smooth. I do not know what the nature of the yellowish component is. I should guess that it is either my organic reaction to the stimulus or something like a memory-color which, under this determination, spreads over the disc or at least spreads over it intermittently. The intermittence does definitely occur; in case of hesitation the hue is now yellowish (complex), and now just something else (a simple green or yellow green). I have thought that this intermittence was exactly like the intermittence that one gets in hearing out the components of a clang."

Conclusions. The results of our first experiment bespeak the simple character both of the intermediate colors and of green; and those of the third experiment also conflict with the alleged composite character of green. The results of the second experiment are equivocal because of inadequacy of method. The results of the first and third experiments are unequivocal; however 'like' a hue may be to neighboring hues, it apparently cannot be resolved by purely psychological methods into these hues as 'components.'

Secondly, the experiments seem to show that, when an observer thinks he 'sees other colors' in a stimulus, he is really perceiving a resemblance to other colors.

Thirdly, a new problem emerges. One does not simultaneously see R and Y in O; the two similarities appear successively, and only with a shift of attitude. There is evidence that the shift of attitude brings with it something more than a shift of clearness, and further experiments must be made to determine what this 'something more' may be.

Fourthly, we find, as Westphal found, that of all hues B is most like itself. The roughness of our materials is, we suppose, at any rate partly responsible for our failure to get a definite confirmation of Westphal's further rank-order: Y, G, R. But it is clear that Westphal's psychological interpretation of individual differences in the determination of *Urfarben* should be put to the test of further experiments.[12]

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[12] Westphal, *op. cit.*, 198, 213 ff. From the phylogenetic point of view, there is a certain plausibility in the hypothesis that B and Y are the only psychologically pure colors, and that R and G are psychological compounds of B and Y. No one, however, so far as we know, has maintained the composite character of R, although the structural likeness of the series B-R-Y and Y-G-B might logically have suggested it. Moreover, the argument from phylogeny must always, in matters of psychological observation, be secondary; the question of this paper, as we said above, is psychological and not biological.

BOOK REVIEWS

Standardization of Tests for Defective Children. By CLARA SCHMITT. *Psychol. Monog.*, 1915, 83. pp. 181.

The tests used are from Healy and Binet, the subjects comprising the kindergarten and first 6 grades of a private school in Chicago, though clinical experience is interwoven with the treatment of them. The clinic must discard time for the most part as an important factor in the measurement of results. Some other measure, preferably a qualitative one, must be substituted for this quantitative one. The clinic must discard rigorous laboratory conditions and adjust its tests to conditions more in conformity with those of everyday life. It must discard such apparatus as requires practice on the part of the subject, or as is not directly connected with the object of the test. A detailed review of the Binet-Simon tests is given, followed by a critical chapter. Their faults may be summed up as (1) the assumption of serial mental development from early childhood to adult age. (2) The omission of tests of socially significant abilities. (3) Failure to distinguish certain innate abilities from a certain expression of them due to age or experience. (4) The series is not an accurate measure of mental development of normal children. (5) The assumption that a defective is quantitatively rather than qualitatively different from a normal individual. The reactions to the Binet-Simon tests are presented in detailed tables. The Healy-Fernald tests were devised with a view to obviating some of these difficulties as well as minimizing the language factor. Correlations of puzzle box and cross line tests with school grade are given. A summary of the standardization of the Healy-Fernald tests presents the relations of these tests to age and grade. There is a chapter on school subjects as tests of mental ability, and the monograph concludes with a division of all the varieties of tests into four classes, each representing a different mental level. The first two belong to the normal grade of mental ability. The mental processes of the upper grades of the defective classes are made upon levels III and IV. The classification could be continued downward to include more fundamental processes such as sensory discrimination as distinguished from such constructive activities as are involved in the lowest level here given.

The Importance of Social Status as Indicated by the Results of the Point Scale Method of Measuring Mental Capacity. By ROBERT M. YERKES and HELEN M. ANDERSON. *Jour. of Educ. Psychol.*, 6, 1915. pp. 137-150.

The authors give a detailed summary of the point scale for testing intelligence. They then present briefly the results of point scale examinations in two city schools which differed radically in the economic and social status of their pupils. The method was to select from the unfavored group an individual of the same sex and of the same or approximately the same age as a given individual of the favored group. 26 boys and 26 girls were in each group. In only one test were the unfavored group superior to the favored. The total scores of the unfavored boys were 21% less, those of the girls 20% less than the favored groups respectively. In the individual records all the unfavored six-year-old boys are below the average of the six-year-old favored boys. In only two cases are six-year-old girls above the average for the favored. Differences in economic or status seem to be correlated with differences in mental capacity so measured which may amount to as much as 30%. It is the conviction of the authors that in the greater part of the practical work of recent mental examination this factor has been neglected. It is proposed to standardize the point scale method so that the mental ability of a subject may be expressed in its relation to the group in which he belongs.

Diagnostic Values of Some Performance Tests. By THOMAS H. HAINES. *Psychol. Rev.*, 22, 1915. pp. 299-305.

In a group of institutional girls the Binet and Point Scale ratings of intelligence set apart three groups consisting of 21 high-grade morons, 16 of doubtful defect and 26 of no defect. Further study of these cases was made by the tests enumerated below. Some of the tests gave significantly different averages in each group and others distinguished some one group from another. A test of moral discrimination is detailed, and though it does not differentiate the groups it suggests to the author the need of studying ethical foundations in the minds of girls. The findings for differential diagnosis of the three groups are summarized as follows: (1) Tests of Value for Both Distinctions, The Picture Form Board, Construction Puzzle (A), The Opposites. 2. Tests Good for Differentiation of the Not Defective from the Doubtful, The Labyrinth (B), The Cross Line (B). 3. Test Differentiating the High-grade Defective from the Doubtful, Visual Verbal Memory, Auditory Verbal Memory. 4. Tests of Doubtful Diagnostic Value, Completion. 5. Tests Showing No Definite Diagnostic Value, Construction Puzzle (B), Learning, Motor Coordination, Moral Discrimination.

Point Scale Ratings of Delinquent Boys and Girls. By THOMAS H. HAINES. *Psychol. Rev.*, 22, 1915. pp. 104-109.

The Yerkes-Bridges point scale for measuring intelligence has certain advantages over the Binet scale in that different groups are more comparable by it; it allows the individual to make credits in any tests while the Binet credit depends upon passing in a narrow range of tests. After making the point scale tests a few short additional tests will complete the Binet rating so that the two may be conveniently compared. In work of this nature done by the author the results are closely parallel, the point scale results, as one would expect from the above, running a little higher. Tables are given showing the comparison of the Binet and point scale ratings in low grade, medium and undefective boys and girls. In the cases classed as "doubtful" where the Binet age is about 11 and the point scale averages considerably higher, about 14, it is felt that the point scale method contributes especially to the analysis of the case.

The Standardization of Knox's Cube Test. By R. PINTNER. *Psychol. Rev.*, 22, 1915. pp. 377-401.

The material is derived from tests of 867 normal children and a few adults, also 463 feeble minded. In the test four cubical blocks "are placed on the table in front of the subjects at a distance of about 2 inches apart. The examiner holds the fifth cube in his hand. He says to the subject, 'Watch carefully and then do as I do.' He then taps the blocks with the fifth cube in a certain definite order and at a certain definite rate (about one tap per second), always beginning with the cube at the child's left or the examiner's right if he is facing the child. He then lays the fifth cube down in front of the child, equidistant between the third and fourth cube, but nearer to the child, and says, 'Do that.'" The blocks were tapped in 12 different ways, designated lines A-J. The complexity of the pattern which can be followed is the index of performance in the test. Curves are given showing the percentages of correct responses for each age and how the patterns ("Lines") compare with each other in difficulty. These curves are quite significant for the author. In defining the age limit of a test he would require about 60% passes but considers that beyond that the best age for placing a test will depend on the shape of the curve showing a percentage of passes at each age. The correct passing of three of his lines, BCD, is delayed until about the fourteenth year. The test may be also evaluated in terms of the number of lines passed correctly at each age. The results gained in this way are about the same as by considering special groups of lines, which is his first described method. In 20 cases the Binet ages were available for comparison and the author considers it surprising that any one test should come so near the results arrived at by a whole series of tests. Two definite types of errors, perseveration and reverse order, are noted. Normal children on the whole do slightly better in the cube test than the feeble minded of corresponding mental age. In summary he gives performances in the test, either by the group method or the actual number of lines method of scoring, which correspond to mental levels of 5, 6, 7, 10, 14 and 16 years. He considers that the child should be credited with the age at which the most difficult combination is passed.

The Adequacy of the Laboratory Test in Advertising. H. F. ADAMS. *Psychol. Rev.*, 22, 1915. pp. 402-422.

The feasibility of predicting through a simple experiment the relative amount of business which different advertisements will bring in is indicated in the writings of Strong and Hollingworth. The general conclusion is drawn that the laboratory test is a satisfactory preliminary in advertising. It was thought that mail order advertising would be worth studying in this connection. A satisfactory measure of the pulling power of an advertisement is not easy. Factors of follow-up, salesmanship and the like seem to make the number of inquiries per insertion the fairest measure. The laboratory tests do not determine whether an advertisement is absolutely good or bad, but only relatively to other members of a series. The figures quoted from the author's experiments show "simply chance resemblance between the results of the laboratory test and the average number of inquiries per insertion, and very little better than chance resemblance between the laboratory test and the business test where profits are used as a measure. . . . With a mail order business it is possible to get returns which are extremely accurate so such advertisements would make the best material for laboratory tests if such tests would only work." College students are however not fair representatives of the mail order purchasing public. The author has been led to question the application of the order of merit method to advertising problems and the previous experiments seem open to attack on several sides. The elaborate keying necessity is impossible with many commonly used articles. Further, the results obtained from laboratory testing are as though the whole hundred per cent of readers were tested instead of the ten per cent who would on the average be interested enough to answer the advertisement. Further, there is the difficulty of any person's telling experimentally just what his actions will be.

Die Abwehrfermente Abderhalden's in der Psychiatrie. By F. Sioli. *Archiv für Psychiatrie,* 55, 1915. pp. 241-271.

A comprehensive digest of the literature is presented, and the author appends some comparative observations, using an organ in different stages of disease. There were 15 cases altogether, of whom 10 were senile dements. The author concludes from the literature as well as from his own observations that the results from the Abderhalden and allied methods are not yet sufficiently clear to be used for purposes of diagnosis and prognosis or pathogenetic theory.

F. L. W.

Proceedings of the Society for Psychical Research, Volume XXVIII. Glasgow University Press, 1915.

The twenty-eighth volume of the Society for Psychical Research (1915) devotes its 657 pages to a "Study of the Psychology of Mrs. Piper's Trance," by Mrs. Henry Sidgwick. According to a list printed at the beginning this brings the total number of pages published by the Society on Mrs. Piper to 2812, the first one coming out in 1890, and yet so dubious is the outcome that Mrs. Sidgwick concludes: "To sum up very briefly my own conclusion about Mrs. Piper's trance, I think it is probably a state of self-induced hypnosis in which her hypnotic self personates different characters either consciously and deliberately, or unconsciously and believing herself to be the person she represents, and sometimes probably in a state of consciousness intermediate between the two. In the trance state her normal powers transcend in some directions those of her ordinary waking self, as is often the case in hypnosis. And further—what makes her case of great importance—she can obtain, imperfectly and for the most part fragmentarily, telepathic impressions. . . . It seems to me impossible at present to prove any theory on the subject." But at the beginning, Mrs. Sidgwick expresses herself thus in capitals (p. 6): "To prevent misapprehension, I am anxious to say emphatically at the very beginning of my discussion that I have no doubt whatever that knowledge is often exhibited in the course of Mrs. Piper's trance utterances which can only have reached her by some supernormal means—by which I mean otherwise than through the ordinary channels of sense," and she inclines to believe that this includes not only telepathy from the living but from the dead. Again, she recognizes that the "communicators" cannot be taken at their face value. "All these points being taken into account, grave doubts are thrown on the genuineness at any time of the dramatic presentation of the communicator. Veridical communications are received, some of which, there is good reason to believe, come from the dead and therefore imply a genuine communicator in the background. But the dramatisation of even genuine communicators, with the whole dramatic machinery employed, is probably merely dreamlike."

If such a wavering stand is the outcome of over thirty years of work, by various individuals, who were from the beginning at least sympathetic with the spiritistic hypothesis, on the "medium" who is generally admitted to give the best returns with the least suspicion of deception, what is the probability of spirit communication?

Amy E. Tanner.

Essays Towards a Theory of Knowledge. By ALEXANDER PHILIP. New York, E. P. Dutton & Co., 1915. 126 p.

This little volume comprises four chapters, entitled respectively, *Time and Periodicity, The Origin of Physical Concepts, The Two Typical Theories of Knowledge,* and *The Doctrine of Energy.* The thesis which the author is concerned to prove is that the fundamental reality is energy and that we must take our clue for the interpretation of experience, not from abstract thought or sensation, but from activity. The two typical theories of knowledge which he combats are (a) the intellectualistic, which "seeks in some way or other to derive the real constituents of Science from the constitution of the cognitive faculty itself" (p. 56), and (b) the sensationalistic, which assumes that the mind is a *tabula rasa* passively receiving the essential forms of reality from the object. As against this it is urged that thought "is an activity which reproduces the activity of things, the activity in which the phenomena of nature arise" (p. 60). The problem of transcendence is solved if we but remember that in action "we are really *part* of a larger *whole.* Our exertional action is *ab initio* mingled in and forms an integral part of the dynamic system in which our life is involved" (p. 64). Sensation is explained as obstructed action, which is hence relational and not photographic in character (cf. pp. 62, 63).

In brief, the author seems to feel that the belief in an all-constituting energy, which is "an alogical, unextended thing-in-itself" (p. 118), somehow makes the problem of knowledge easy of solution. Just how this comes about the reviewer is unable to state. The author shows no real appreciation of historical solutions of the problem, nor does he make any effort to face the difficulties, in connection with this subject, which are before the philosophical public at the present time.

University of Illinois, B. H. BODE.

Human Motives. By JAMES JACKSON PUTNAM. Boston, Little, Brown & Co., 1915. pp. xvii+179.

In a little book of contributions to social and religious psychology a well-known authority on medical subjects turns his attention to those motivations of conduct which long years of keen observation and the more recent psychoanalytical investigations have revealed. He finds that the conflict of our rational and emotional impulses resolves itself into an interaction of two motives, the constructive and the adaptive. In their creative enterprises men are moved by aspirations which aim at personal gratification and advancement. These two general classes of motives have a historical development in the individual as well as in the race and lend themselves to study either by the rational method in terms of the philosophy of religion or by the genetic method in terms of psychoanalysis. While religious faith points to the presence of a group of ideals toward which man is constructively working and in terms of which he is acknowledging an obligation to a deity immanent in the universe, psychoanalysis shows the presence of unconscious tendencies which, if not properly controlled and guided, often militate against these natural aspirations.

An occasion is therefore given for sketching the history of the psychoanalytic movement and for an outline of its main principles, methods and aims. The reviewer feels that the exposition is here clearer and more adapted to the class of readers for which the *Mind and Health Series* was intended. Above all—and this is not commonly done—the interpretation of the Freudian attitude, especially that of Freud himself, seems to be just and unbiased. With all that the author has previously said in the book about religion, he leaves the doctrines of Freud singularly free from religious or philosophical conceptions. In the chapter on the "educational bearings of psychoanalysis," the author emphasizes the fact that teachers and parents ordinarily cannot hope to become psychoanalysts, but they may study ways in which the main propositions of the method "can be applied to children at large, in the schoolroom and at home." He stands in agreement with Münsterberg when he indicates the harm which too much intimate conversation with children can do[1]; he suggests, however, that earnest inquirers should not be scornfully rebuked. Abnormalities are but prolonged or exaggerated forms of disturbance which occur in the normal mind in lesser degree, and are often caused by inhibitions of a social nature with no proper outlet for dissipation. In essentials the author's ideas are suggestive of those of Holt in a recently published volume on the same subject: psychoanalytical treatment is not so much a cure as a reëducation.[2]

The last two chapters are concerned with the problem of the readjustment of the inner life of the individual to his social and spiritual environment. The possibilities of individual achievement are surprisingly great; but are much curtailed if not rightly directed toward a larger and ever deepening influence on humanity.

CHRISTIAN A. RUCKMICH.

Sleep and Sleeplessness. By H. ADDINGTON BRUCE. Boston, Little, Brown & Co., 1915. pp. ix+219.

In the opening chapter, the causes of sleep are discussed. The author lays much stress on the experimental reports of Sidis, Verworn, and Huebel to the effect that sleep does not so much depend on physiological conditions as on a hypnoidal state of mind. Expressed in other words, "when we sleep, we do so because our consciousness is no longer stimulated by a sufficient variety of sensations to keep us in a waking state," or "variety itself has temporarily become monotonous, and we fall asleep." Coriat's criticism that the "diminution of peripheral stimuli from the muscles to the brain, produced by the act of muscular relaxation," is responsible for the initiation of sleep, is accepted because Coriat admits "that the muscular relaxation itself may result from a relaxation of attention." The author gives numerous and apt illustrations in support of his "monotony theory." His statements flatter psychology, but do they state the whole truth? Any simplification of scientific problems for the purpose of popular exposition must naturally make light of the difficulties involved. Sleep is more likely a psychophysical phenomenon; it may be considered from the side of mind, or of body. To say that mental conditions cause bodily conditions even indirectly, or *vice versa,* or to speak of causal conditions and of accompanying conditions, begs the whole question of interactionism.

Under the caption of *The Mind in Sleep,* with a wealth of appropriate, sometimes personal, examples, the theory that dreams may be caused by past experiences or by present stimuli, is outlined. The Freudian hypothesis of the 'wish complex' is indorsed with reservations.

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[1] Münsterberg, H. *Psychology and Social Sanity,* 1914. pp. 3ff.

[2] Holt, E. B. *The Freudian Wish,* 1915. pp. 100ff.

Dreams which have been interpreted in terms of supernatural influences are classified into four groups: (1) the inspirational dream, giving rise to unusual achievement; (2) the somnambulistic dream, in which difficult problems are solved; (3) the revelational dream, in which hidden objects are brought to light; and (4) the telepathic dream that conveys messages from a distance. These are all explained in terms of 'subconscious mentation' and 'subconscious perceptions.'

The last three chapters consider the disorders of sleep and the causes and treatment of sleeplessness. A note of warning is sounded with respect to the arousal of childish fears and the consequent danger of inciting nightmares and somnambulistic disturbances. Insomnia as a rule is nothing more than "a bad habit of mind" and is induced by emotional disturbances which can be avoided. The physical factors are again secondary to the psychical, and suggestion, in its various forms, is the best palliative medium.

The book abounds in that form of illustrative material which will appeal to the average person who reads the work for the sake of personal benefit to be derived. Also the general reader will learn much from its pages. In this way it will carry out its stated mission "to extend knowledge of the important discoveries affecting individual and social welfare." The only negative criticism which occurs to the reviewer is the natural one: that in the endeavor to give weight to the helpful suggestions the authority of psychology has been slightly overemphasized.

University of Illinois, CHRISTIAN A. RUCKMICH.

BOOK NOTES

The psychology of relaxation. By George Thomas White Patrick. Boston, Houghton Mifflin, 1916. 280 p.

Professor Patrick has for many years been engaged upon this subject, and brings together here five of his studies with modifications up to date, viz., the psychology of play, of laughter, of profanity, of alcohol, and of war, four of these having been reprinted elsewhere but revised here. The general thesis, which is original with the author, is that play is not only relaxation but reversion to activities in which the race has been engaged in earlier times, or to the psychokinetic equivalent of these activities. This goes distinctly beyond Groos or anyone else in this field and greatly reinforces all the arguments for the practical uses of play in education.

A beginner's psychology. By Edward Bradford Titchener. New York, Macmillan, 1915. 362 p.

The author tries to give us here the kind of book he would have found useful when he was beginning the study of psychology, nearly thirty years ago. The book is almost entirely different from the author's older "Primer"; at least, he tells us, "There is change all through; every paragraph has been rewritten. The greatest change is, however, a shift of attitude; I now lay less stress than I did upon knowledge and more upon point of view." Most of our readers are familiar with the old edition, and the best we can do here is to give the topics of this book's twelve chapters: Psychology, what it is and what it does; sensation; simple image and feeling; attention; perception and idea; association; memory and imagination; instinct and emotion; action; thought; sentiment; self and consciousness; and dreaming and hypnosis.

The Yoga-system of Patañjali, or the ancient Hindu doctrine of concentration of mind. Translated from the original Sanskrit by James Haughton Woods. (Harvard Oriental Series, ed. by Charles Rockwell Lanman, vol. 17.) Cambridge, Harvard University Press, 1914. 384 p.

This important work cannot fail to appeal strongly to psychologists, for the Yoga-system has come into great prominence in recent times. It gives us the ancient Hindu doctrine of the concentration of mind, embracing the mnemonic rules and their comment and explanation. A very valuable introduction of some thirty pages explains the reasons for taking up this work, its difficulties, and discusses authorship, comparison of philosophic concepts and their non-conformity with tradition, the date of the Sūtras, which is placed between 300 and 500 A. D., and, most valuable of all, a brief summary concerning (1) concentration, (2) means of attainment, (3) supernormal powers, and (4) isolation.

Fundamentals of sociology, with special emphasis upon community and educational problems. By Edwin A. Kirkpatrick. Boston, Houghton Mifflin Co. (c. 1916.). 291 p.

This book is intended as an introduction to the subject, and deals with the evolution of group action; biological view of human development; a psychological view of behavior and needs; economic, protective, recreative, cultural, social, moral, religious, and educational needs and activities; elementary, intermediate, secondary and higher needs and activities. Finally he deals with the family as a social group and as an institution; the community and its functions; general, economic, protective, recreative, cultural, social, religious and educational community studies.

Who is insane? By STEPHEN SMITH. New York, Macmillan, 1916. 285 p.

In 1882 the author was appointed State Commissioner in Lunacy of New York. At that time the insane in custody were about twelve thousand. The author introduced a training school for attendants, a state commission in lunacy, the removal of the insane from county to state care. These reforms raised the standard of care and treatment throughout the state. The chief topics are, who is insane and what is insanity; critical periods of life predisposing to insanity; care and treatment; the lesson applied to the feeble-minded and criminal; and finally, a new constitution—the dawn of a better day, with one section on eugenics.

The effect of age on habit formation in the albino rat. By HELEN B. HUBBERT. (Behavior Monographs, Vol. 2, No. 6, 1915.) Cambridge, Henry Holt & Co. 55 p.

This is a valuable paper and shows that young rats learn the maze more readily than old ones, that the time increases with age, that the most rapid stage of habit formation occurs lower in the learning process of young animals than of older ones, that for young rats sex is negligible while among older ones the males learn more rapidly, that the absolute time for the females is lower than for the males, that there is no difference between night and day learning, that continued practice after the habit is learned does not increase final efficiency, and that the importance of adequate rest is plain.

Psychological effects of alcohol; an experimental investigation of the effects of moderate doses of ethyl alcohol on a related group of neuro-muscular processes in man. By RAYMOND DODGE and FRANCIS G. BENEDICT. With a chapter on free association in collaboration with F. Lyman Wells. Washington, Carnegie Institution, 1915. 281 p.

This is a very exhaustive and thorough-going study, with 32 illustrations, which, after describing the plan of the investigation, explains the effect of alcohol on the simplest neural arcs, then on complex ones, and in later chapters on free associations, process of memorizing, on the sensory threshold for faradic stimulation, on motor coördinations, on pulse-rate during mental and physical work experiments, while the last chapter is devoted to summaries and correlations.

La pensée et la polyglossie; essai psychologique et didactique. Par IZHAC EPSTEIN. Lausanne, Librairie Payot et Cie, n. d. 220 p.

This very interesting monograph discusses the effect of the knowledge of several languages upon the mother-tongue. The chapter headings, anglicized, are as follows: automatism in languages by polyglots; inner language and polyglottism; the negative influence of present factors favorable in a language; the auditive influence; intercalation; diverse identification of the same ideas in different languages; sequence of ideas; variation of interpretive activity; practical applications.

Individuality in organisms. By CHARLES MANNING CHILD. Chicago, University of Chicago Press (c. 1915). 213 p.

The chief topics are the problem; theories of organic individuality; metabolic gradients in organisms; physiological dominance in the process of individuation; the range of dominance, physiological isolation, and experimental reproduction; discussion, conclusions, and suggestions. It is the statement of the conceptions of the nature of organic individuality which have developed in the writer's mind during the last fifteen years as a result of investigating the simpler processes of reproduction in lower animals.

The American college; a series of papers setting forth the program, achievements, present status, and probable future of the American college. With introduction by William H. Crawford. New York, Henry Holt & Co., 1915. 194 p.

This is an admirable collection of recent important papers upon this subject by President Faunce of Brown, Professor Shorey of Chicago, Dean Haskins of Harvard, Professor Conklin of Princeton, Presidents Rhees of the University of Rochester, Thwing of Western Reserve, Finley of the University of the State of New York, Few of Trinity, Slocum of Colorado College, Meiklejohn of Amherst, and Commissioner Claxton.

The crowd in peace and war. By SIR MARTIN CONWAY. New York, Longmans, Green, 1915. 332 p.

This book deals with kinds and nature of crowds, crowd units, continuity, instincts, compellers, exponents, representatives, organization, relations to government, liberty and freedom, education, morals, religion, overcrowds, etc. Only the last five chapters are devoted to war, beginning with page 265, its cause and cure, contest of ideals, the crowd at war, the value of the crowd, and the just mean.

Einführung in die Psychologie. Von E. v. ASTER. Leipzig, B. G. Teubner, 1915. 119 p.

This work is divided into four parts: first, generalities on the essence of the psychic; second, sensation and perception; third, the life of *Vorstellung;* fourth, feeling and will. It seems an admirable little compend but the script is German, and even the largest font of type is small, and the other sizes are simply ruinous to the eyes.

Harvey's views on the use of the circulation of the blood. By JOHN G. CURTIS. New York, Columbia University Press, 1915. 194 p.

The author of this book has been well known for many years as an able professor of physiology in Columbia University, and he has made here an admirable study of his subject in twelve chapters, with four illustrations.

Honesty; a study of the causes and treatment of dishonesty among children. By WILLIAM HEALY. (Childhood and Youth Series, edited by M. V. O'Shea.) Indianapolis, Bobbs-Merrill Co. (c. 1915). 220 p.

This is an excellent and needed study, to which the author brings a number of years of rare opportunity and excellent training and ability. The following heads of chapters will give the best idea of the nature of the book: age of moral development; home conditions and parental behavior;companionship; discipline; amusement and adventure; habits—mental, physical and social; physical conditions; abnormal mentality correlated with stealing; impulsions and obsessions.

Being well-born; an introduction to eugenics. By MICHAEL F. GUYER. (Childhood and Youth Series, edited by M. V. O'Shea.) Indianapolis, Bobbs-Merrill Co. (c. 1916). 374 p.

This is a valuable book by a very competent hand. The heads of the chapters are as follows: heredity; the bearers of the heritage; Mendelism; Mendelism in man; are modifications acquired directly by the body inherited; prenatal influences; responsibility for conduct; mental and nervous defects; crime and delinquency; race betterment through heredity, with glossary and references.

The rhythm of life. By CHARLES BRODIE PATTERSON. New York, Thomas Y. Crowell Co. (c. 1915). 303 p.

The more important chapters here are on music of other climes, energy motion—vibration, music and colour tones, colour values, music—a compelling power, joy rhythm—the dance, nature and art in singing, colour tonics, keynote to health—harmony, cosmic consciousness, prophets of the invisible, life's love melody, music and character, music and education, a refining influence, musical therapeutics. The volume moves largely within the domain of New Thought, to which it makes an important and interesting contribution.

The transfer effects of practice in cancellation tests. By MELVIN ALBERT MARTIN. (Archives of Psychology, No. 32, August, 1915.) New York, Science Press. 68 p.

After discussing previous investigations and descriptions of material and procedure, the writer treats, first, in the preliminary investigations, of reaction time and cancellation; under present investigation, he treats of practice and test material, and procedure; third, treatment of data in the practice and test series. Finally comes the interpretation of results, in terms of speed and accuracy of performance, when corrected for errors, with summary and general conclusions.

The intellectual status of children who are public charges. By J. L. STENQUIST, E. L. THORNDIKE and M. R. TRABUE. (Archives of Psychology, No. 33, September, 1915.) New York, Science Press. 52 p.

This book describes the children measured and the tests used, viz., the Stenquist test of mechanical ability, or construction test, the Trabue completion test, Binet-Simon and reading tests; the results; heredity versus environment as the cause of the low intellectual status of dependent children.

The relation of quickness of learning to retentiveness. By DARWIN OLIVER LYON. (Archives of Psychology, No. 34, January, 1916.) New York, Science Press. 60 p.

The writer first gives a brief history of the topic, then describes method, problem, material, scoring, classes of subjects tested, and finally gives his results in the form of tables, time of initial learning, interval between learning and reproduction, amount retained, intellectual standing, social standing, age, sex, with summary and recapitulation of the main results.

✓ *The overcoming of distraction and other resistances.* By John J. B. Morgan. (Archives of Psychology, No. 35, February, 1916.) New York, Science Press. 84 p.

After a description of the apparatus, the experiments with time and accuracy records are explained, responses to noise, as shown by breathing, comparison of quiet and noisy periods with respect to effort, comparison of the effect of various noises, relative significance of changes in time and force, with general summary and conclusions.

Completion-test language scales. By Marion Rex Trabue. New York, Teachers College, Columbia University, 1916. 118 p.

This work is based upon completion tests from over thirteen thousand public school pupils, and was done under the guidance of Professor E. L. Thorndike. It deals with language scale A, additional language scales, and calculation of the difficulty of test sentences.

Educazione dei deficienti. By Sante de Sanctis. Milano, Dottor Francesco Vallardi, n. d. 300 p.

After a chapter on nomenclature and classification, the author discusses individual defectives, various tests, physical and psychopathic, causes, intelligence, volition, types of abnormality, grades and scales, organization of a school for investigation of defectives and their education, intellectual and moral, with copious biographic notices.

The victorious attitude. By Orison Swett Marden. New York, Thomas Y. Crowell Co. (c. 1916). 358 p.

Of the sixteen chapters of this work the more important seem to be, doubt the traitor; making dreams come true; a new rosary; making yourself a prosperity magnet; the suggestion of inferiority; have you tried love's way; the triumph of health ideals; preparing the mind for sleep; how to stay young; our oneness with infinite life.

✓ *Patience Worth; a psychic mystery.* By Casper S. Yost. New York, Henry Holt & Co., 1916. 290 p.

Healy's "The Individual Delinquent." By F. L. Wells. (Reprinted from the Psychological Bulletin, Vol. XII, No. 7, July 15, 1915.) p. 274-277.

Le mécanisme des phénomènes hystériques; esquisse d'une thèorie psychophysiologique de l'hystérie. Par Albert Salmon. (Reprinted from Nouvelle Iconographie de la Salpêtrière, Nos. 3 and 4, 1915.) 16 p.

L'emozione; studie di psicologia generale. By Alberto Salmon. (Reprinted from Quaderni di Psichiatria, Vol. 11, No. 9-10.) 26 p.

Just be glad. By Christian D. Larson. New York, Thomas Y. Crowell Co. (c. 1912). 64 p.

On books and reading; outline of a course of lectures for nurses in hospitals. By Edith Kathleen Jones. (Reprinted from American Journal of Insanity, Vol. LXXII, No. 2, October, 1915.) p. 297-303.

The hospital library—books for the patients. By Edith Kathleen Jones. (Reprinted from The Modern Hospital, Vol. V, No. 4, October, 1915.) 12 p.

A brief bibliography of books in English, Spanish and Portuguese, relating to the republics commonly called Latin American, with comments. By Peter H. Goldsmith. New York, Macmillan, 1915. 107 p.

Thought-content and feeling. By Knight Dunlap. (Reprinted from the Psychological Review, Vol. XXIII, No. 1, January, 1916.) p. 49-70.

Color theory and realism. By Knight Dunlap. (Reprinted from the Psychological Review, Vol. XXII, No. 2, March, 1915.) p. 99-103.

A new measure of visual discrimination. By Knight Dunlap. (Reprinted from the Psychological Review, Vol. XXII, No. 1, January, 1915.) p. 28-35.

The shortest perceptible time-interval between two flashes of light. By Knight Dunlap. (Reprinted from the Psychological Review, Vol. XXII, No. 3, May, 1915.) p. 226-250.

Ricerche sperimentali sulle illusioni dell' introspezione. By Enzo Bonaventura. (Reprinted from Psiche, Vol. IV, Nos. 1, 2 and 3, 1915.) 133 p.

L'insegnamento della psicologia all' estero. By Enzo Bonaventura. (Reprinted from Bollettino della Associazione di Studi Psicologici, Vol. I, No. 4, and Vol. II, No. 1.) p. 69-100, and p. 1-10.

The academic status of psychology in the normal schools. Report of the committee on the academic status of psychology, American Psychological Association. Princeton, N. J., Printed by the Committee, December, 1915. 33 p.

A LABORATORY NOTE

Since the beginning of the present war, there has been more or less difficulty in having orders filled for apparatus or for parts of apparatus, ordered from German instrument firms. Hence the replenishing of the laboratory stock of such things as rubber sleeves for the Lehmann plethysmograph becomes rather of a problem.

We attempted to solve this, in the case of the rubber plethysmograph sleeve, by having a manufacturing rubber firm dip some sleeves especially for us from our own mold; but we were unable to find a firm who was willing to consider such a small order. Hence it became necessary for us to find a substitute for this form of rubber sleeve. The rubber surgical glove or the long rubber maternity glove at once suggest themselves. These, however, are both expensive and it will also be found that the subject can insert his hand and arm into the instrument only with a great deal of difficulty, even though a large sized glove is employed.

The bladder spinal ice bag was suggested to us, however, as a substitute for the rubber plethysmographic sleeve, and this proved to be an excellent substitute indeed. This bladder spinal ice bag is a long narrow rubber sac, without joints and closed at one end. The open end is reinforced by a double thickness of rubber and is hence slightly narrower than the rest of the bag. Hence we have here a seamless rubber bag of almost identical shape to the customary sleeve. This bladder spinal ice bag is not to be confounded with the laced spinal ice bags of English design.

Bladder spinal ice bags, indeed, recommend themselves for constant use with the Lehmann plethysmograph. They are very quickly and readily obtainable, and indeed, are kept constantly in stock by any first-class wholesale rubber or drug firm. Hence the laboratory need not lay in a large supply which may rot before use. The slightly narrowed open end of the bladder spinal ice bag fits over the open end of the metal sleeve so tightly that there is small likelihood of leakage at this point. Furthermore the walls of this ice bag are slightly thinner than those of the customary sleeve and hence should give a more delicate record. Finally, the bladder spinal ice bag recommends itself for use by the small price for which it may be purchased; a price usually ranging from 30 to 40 cents apiece. S. W. F.

Oswald Külpe, 1862-1915. The death is announced, at the age of fifty-three years, of Professor Külpe of Munich, equally well known to psychologists and philosophers because of his many publications in both fields. Külpe's elementary text-books of psychology and of the history of philosophy are very widely read and each has gone through a number of editions. He has contributed very greatly to the theory and practice of introspection, and is, indeed, the father of the so-called Wurzburg method which grew under his direction while he was professor at the University of Wurzburg, and which has contributed so largely to the imageless thought movement. At the present time Professor Külpe was re-writing his *Grundriss der Psychologie* from this point of view. Among his published works are: *Grundriss der Psychologie*, 1893; *Einleitung in die Philosophie*, 1895; *Welche Moral ist heutzutage die beste?* 1900; *Die Philosophie des Gegenwart in Deutschland*, 1904; *Immanuel Kant, Darstellung und Würdigung*, 1907; *Erkenntnistheorie und Naturwissenschaft*, 1910; *Psychologie und Medizin*, 1912; *Die Realisierung: ein Beitrag zur Grundlegung der Realwissenschaften*, 1912. A selection of some of Külpe's more important articles show his tremendous breadth of interests: *Zur Theorie der sinnlichen Gefühle*, 1887; *Aussichten der experimentellen Psychologie*, 1894; *Zur Lehre von der Aufmerksamkeit*, 1897; *Ueber den associativen Faktor des ästhetischen Eindrucks*, 1899; *Zur Frage nach der Beziehung der ebenmerklichen zu den übermerklichen Unterschieden*, 1902; *Ueber die Objektivirung und Subjektivirung von Sinneseindrücken*, 1902; *Ein Beitrag zur experimentellen Aesthetik*, 1903; *Ueber die Beziehungen zwischen körperlichen und seelischen Vorgängen*, 1908; *Pour la psychologie du sentiment*, 1910; *Contribution to the History of the Concept of Reality*, 1912; *Ueber die moderne Psychologie des Denkens*, 1912.

THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded by G. STANLEY HALL in 1887

VOL. XXVII JULY, 1916 No. 3

THE RENEWAL OF FRENCH THOUGHT ON THE EVE OF THE WAR

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Some time ago, a French critic, Mr. Blum, in a striking article in *La Revue de Paris,* wrote to the following effect: French thought since the time of the Revolution has evolved according to a great curve which to-day returns to its starting point, thus forming a vast circle. We stand to-day almost exactly where our ancestors stood about a century ago after the transition period ending with the fall of Napoleon and beginning with the Restoration in 1814 and 1815. Artists, poets, writers, philosophers of that age had assumed an attitude of protest against that rationalism and scepticism which was the characteristic of the eighteenth century. Now our artists, our poets, our writers, our philosophers of the present age show the same disposition towards the Rationalism—or, we sometimes call it Realism, the word does not matter—which is the characteristic of the second half of the nineteenth century. A wave of idealism, of religiosity, sweeps the world of thought almost as if nothing had happened during the last hundred years. Our generation is ready for the *Méditations* of a new Lamartine, for the *Poèmes Bibliques* of a new Vigny, for the *Confessions* of a new Musset, for the *Contemplations* of a new Victor Hugo.

And indeed, to-day we have already witnessed much more than was announced in that curious prophecy. Some of those who are unmistakably the standard-bearers of the morrow have offered *more* than the Romanticism of a hundred years ago, *more* than the vague "tourment de l'infini" of Vigny and Musset, *more* than the semi-pagan religiosity of Lamartine, *more* than the agnostic spiritualism of Victor Hugo, *more* even than the merely esthetic Christianism of Chateaubriand, or the rationalistic Protestantism of Madame de Staël. This is no less than the traditional Catholicism of France, which is now revived in the writings of some of the prophets of the younger generation, the orthodox Catholicism of the priest, the Catholicism of the repenting sinner, the Catholicism of the France which had been honored by the title of "Eldest daughter of the Church." And therefore,—one ought to emphasize it too—this is *more* than the pseudo-Catholicism of a Huysmans, or of a Maeterlinck; those two men, who at most may be called precursors, but nothing more, are already far behind.

And this thoroughgoing change has taken place with an astounding rapidity.

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Of course the attempts had been made before to lead into new channels the thoughts of the youth of France. Such eloquent and clever and subtle writers as Brunetière, Bourget, and Lemaître had been earnestly suggesting: "Let us go back to Rome." But, who was listening, and who heard them? Only a limited number of people belonging to the sheeplike class, and who did so because they always feel a vague fright at whatever progressive theories might disturb their bourgeois quietude. When followed by others, moreover, these men were followed because of their fame rather than because of their ideas. And indeed, is it not remarkable that the call of men standing so high in public esteem did *not* appeal for so long a time to these younger people on whom the ideal of the future depends? . . . While today, all at once, with practical unanimity, a whole generation, all that is young and strong and hopeful, rushes with enthusiasm into the new path—no! this is wrong: into the path that had been deserted by the class called the "intellectuels"!

The movement was as general as it was rapid. It stands to reason that such a change would not be confined to literature.

It found a fertile soil in the domain of arts, for example. Futurists and cubists have created quite a stir. But people of wisdom have remarked long ago that drums make noise because they are empty. *Others* were toiling who touched the hearts, and their work stands out calm and beautiful after the loud uproar over sensational products has subsided. As we know, there are usually at each Salon in Paris, one or two paintings which seem to focus the interest of the visitors and arouse discussion. At the last Salon d'Automne before the war—which was held in 1913—one of these pictures singled out, as if instinctively, by the public was an "Annonciation" by Maurice Denis. Denis was already the painter of "L'exaltation de la croix" and has reminded many people of the sweet and pure conceptions of Fra Angelico. Another picture admired and discussed was Georges Desvallières' "Le bon larron": it is the old scene of the Crucifixion; the "bon larron" is casting a glance full of gratitude towards a livid Christ, while the hideous face of the "mauvais larron" betrays the great terror suggested by the moral suffering of the sinner; Desvallières reminds one of the school of those Gothic painters who emphasize the torments of the religious soul. The interpretation of the two artists is certainly different, almost in opposition; still each is the interpretation of one and the same idea: the Christian idea, the Catholic idea.

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And why should we not speak here of Rodin, up to now the incomparable sculptor of physical forces, of the chaotic power of the universe, and who, just a few months before the European conflagration, seizes a pen and feels that he has something to say? What sort of inspiration is going to take hold of the Goethe of sculpture? His work bears the titles *Les Cathédrales de France*—a prophet's book, one might almost say, especially in these pages where he sings a song of piety to the beauty and to the sanctity of the cathedral of Reims. He was the man, if any, who could give it back to us a little by his stirring description.

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Being so widely known, short mention only is necessary here of the great idealistic movement in philosophy, and which has been more or less aptly baptized "Bergsonism." Let us say here, however, that at the beginning Bergson himself, as if he was not aware of the deep renewal of thought of which he was made the chief representative, had remained very cautious both in his books and his lectures. For years he was satisfied with a purely negative attitude: he rebuked the haughtiness of scientists, and denounced the shortcomings of a popular rationalism, that was all; after a while he consented to walk hand in hand with the representatives of a rational pragmatism; and finally it was in his New York lectures that he made his first positive utterances regarding God, freedom of the will, and immortality—lectures, which, by the way, were never published. The last we have from him before the war is an interview of about one year ago, when he admitted to a Jesuit Father, the Père de Tonquedec: "De mon oeuvre se dégage nettement l'idée d'un Dieu créateur et libre." (The logical inference from my works is plainly the idea of a creating and free God.)

And is it not remarkable that the wise man of Serignan,[1] the great God-fearing naturalist, J.-H. Fabre, greatly admired long ago by Darwin, but who had remained almost completely ignored except by a very small circle of specialists in entomology, should have become *suddenly* in recent years a world celebrity? Scientific associations were now eager to honor him, the President of the French Republic paid a solemn visit to him in his retreat—and last but not least, it is from him that Bergson borrowed the most striking arguments in support of a non-materialistic universe (in his last volume, *L'évolution créatrice*).

* * *

The same attitude prevails with recent critics in various domains.

It may be simply a matter of coincidence that one of the greatest works in the field of literary criticism and erudition, just completed, should bring back an interpretation, more religious even than that of the very catholic Léon Gautier, to account for the origin of the great national French epics—*La Chanson de Roland* and others—: we refer to the theories of Joseph Bédier, of the Collège de France, in his imposing *Légendes Epiques;* the French epics, according to him, are simply pilgrim songs. . . . This happens at the very moment when the famous abbé Loisy, elected to a chair of the University of Paris to profess, unhampered by the Church, his liberal and anticlerical views, falls more and more into oblivion.

It may be a matter of coincidence too, but just at this time cannot well escape notice, that while erecting a monument to the most enthusiastic disciple of the Renaissance, Rabelais, in the form of an elaborate edition of his works, the great scholar Abel Lefranc, right beside the one to Rabelais, erects another monument to Calvin in publishing a new edition of the *Institution Chrétienne.* A luxurious edition of Stendhal's works, now in process of publication, is at present widely advertised in Paris; but for one very profane author, how many Catholic authors, whose works are also published anew, and receive a generous welcome back to fame by our generation, Joseph de Maistre, Hello, Lamennais, Montalembert, etc.!

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[1] He died in the first year of the war.

It is a matter of mere coincidence perhaps also, that just last year there should have been a demand for a new edition of Schuré's profoundly mystical book, *Les Grands Initiès,* and that one of the first volumes edited in the modern popular collection Nelson—and one of those which sells best—should be that exquisite *Introduction à la Vie Dévote* by Saint-François de Sales . . . Saint-François—and this other remarkable boom of the other Saint-François, the great Saint-François, Saint-François, d'Assise: A Frenchman started it, Paul Sabatier, who was followed by the Père Edouard d'Alençon, who was followed by the Dane Joergensen, who was followed again by a Frenchman, Lafenestre, of the French Institute, providing us with the volume needed for the public at large and which reads like a beautiful and elevating novel, *La Légende de Saint-François d' Assise.*

To a man who resists drawing hasty conclusions, it might seem a mere coincidence also that the *Vie de Jeanne d'Arc* which Anatole France chose to write in 1908 should be the first book by this idol of French letters to meet with a cold reception, while three years later Hanotaux, whose fame does not compare with that of France as a writer, and who claiming for the touching heroine the right to be "divine," opposed his Jeanne d'Arc to that of the great man, should score a success with his book.

* * *

And if all this were merely a matter of coincidence, it is *not* in any case a matter of coincidence that the winter before the war, the book of the season in France, the book that one must read, the book that everybody was talking about, was a *Saint-Augustin.* This literary event is truly typical of the change that has taken place in the philosophical attitude of the writers who find favor with the public.

The author of *Saint Augustin,* Louis Bertrand, born a little over forty years ago, belongs to that élite of Frenchmen called the "Normaliens."[2] After he had completed his studies in Paris, he was sent as professor of literature to Algiers. He had written for his Doctor's degree on *La fin du classicisme et le retour à l'antiquité.* In Africa he became directly acquainted with the country and the remains of the civilization which had inspired the authors he had studied. The equatorial skies, the dazzling, yet soothing landscapes of the southern coast of the Mediterranean impressed him with their peculiar charm. He wrote novels, and books of travel filled with the inspiration of the "mirage oriental." And finally, combining his erudition with his love for African life he decided to revive with his pen the struggle of Christian civilisation in Africa just before the fall of the Roman empire—a page of history strangely neglected by modern scholars. The result was that stirring book *Saint-Augustin.*

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[2] That is to say that he was at one time a pupil of the "Ecole Normale Supérieure" an institution where the French government provides all the necessities of life and the training by the very best professors of France for a few young men, winners in competitive examinations held yearly through France. The selection is made very

The reader cannot resist the charm of Bertrand's vision of those cities representing a civilization that has been wiped out of existence by historical events: Sagasta, the dwelling place of Augustin's parents, "douce et riante" like France; voluptuous Carthage, with a civilization more refined than that of Rome, and so full of danger for the pleasure-loving and intelligent student Augustin; the corrupted imperial Rome, with her mixture of people and races, and which in spite of her gilded temples and imposing buildings one could compare to a heap of manure feeding by its very rottenness the seed of Christianity; Milan, where the austere and keen Ambrosius persuaded the northern tribes who had invaded Italy to serve the Christian God, and where Augustin himself, the teacher of pagan rhetoric, opened finally his heart to the gospel of truth; Hippone, then, with her convent, and her girdle of walls, Hippone, witness of the holy life of the greatest of Christian bishops.

And how full of life is the picture of the household where the Christian hero spends his first years, where he saw the Christian woman, Monique, conquer the brutality of a husband and the acrimony of a harsh mother-in-law, "by dint of patience, sweetness of manners and charity." Later in the book, we have the anguish and the prayers of the same

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much in the same manner as that of the Rhodes Scholars in English countries. The Ecole Normale is responsible for the training of a remarkable proportion of illustrious men in France. Many consider it a great blow to the prestige of the venerable school, however, that a few years ago it was decided that the Normaliens would no longer have their own classes, but simply attend the Sorbonne—thus leaving the school to provide only lodging, food, library and laboratories; *plus,* of course one thing which means very much, the daily intercourse with the picked youth of France in various fields of knowledge.

holy woman for the salvation of her beloved son; and after she has seen him conquered, in Milan, by the Man of Genezareth, her ecstatic death in Ostia. Nothing could be more lucid, more sober and more stirring than the chapter relating the final crisis, nothing more gloriously radiant than the picture of the famous scene of Christ calling in the Garden. And it takes indeed the best in the excellent French literary artist to lead a profane reader so smoothly through the different states of minds which were to bring Augustin at last to the Christian God: that is to say, from the religious atmosphere of the paternal household, through Manicheism, through neo-platonic scepticism, through agnosticism, finally to the Paulinian faith, at once strong, severe and human. All this is interesting.

But what is most interesting of all, is the spirit which animates the book. In vain would the reader try to discover traces of the sceptical and haughty attitude of the scholar which was the fashion yesterday; or of an author concerned merely with objective treatment, i. e., looking at it, as one might say, only from the outside; or, still less, of the purely rationalistic philosopher who would consider it a disgrace if he did not disintegrate by analysis all the feelings and emotions of his hero. On the contrary. It is with the most sympathetic curiosity that the author bends over his Saint Augustin—a sympathy so true and so sincere that he cannot help seeing another self in that man who has been tormented for so many years, longing and struggling, but who has found truth finally. Augustus lived sixteen times a hundred years ago, yet the problems which he faced, the anguish he went through were the same as ours. And the celestial happiness which he found may be ours too: "That life (of Augustin)—writes Mr. Bertrand—and the age that witnessed it, remind us of our own age and of ourselves. The recurrence of similar circumstances has brought about the recurrence of situations and characters of a similar nature. It is almost our portrait. We are extremely near concluding that at the present hour there is no more timely subject than Saint-Augustin." He goes on explaining: We live in an age when no thinking being can afford to assume a purely contemplative attitude towards life. No age more than ours needs to recall Christ's words: "I came not to send peace on the earth, but a sword"; and every one of us must, after having consulted his conscience, say a *Yes* or a *No*. So it was in the fourth century; so it was with Saint Augustin: "People hesitated between the beliefs of yesterday and the faith of the morrow. Augustin was one of those who had the courage to choose, and who after having chosen this faith, proclaimed it without weakening."

Another feature of the book explains the unusual appeal it made to modern French readers. Mr. Bertrand recalls Pascal, who like Saint Augustin—and to a great extent a continuator of Saint Augustin—found all his religious inspiration in the fact that he was so completely overawed by the idea of God. But Mr. Bertrand explains why Pascal, although he lived only three centuries back, seems much farther from us than Saint Augustin who is sixteen centuries old. Pascal speaks first of all of an infinite God, and of a God who hates sin, of a God of justice and of austerity; while Saint Augustin's God is not alone just, not before all else austere; He is primarily good, He is tender, He is the Heavenly *Father*. Now, we are tired of the cold impersonality and impassibility of science, and the God of Pascal reminds us too much of that. Our hearts are thirsty for a God, but for a God who would be—if one may say so without sacrilege—like Saint Augustin's, more ***human:*** "The judgment passed by Pascal on our human fragility knows no sympathy. The God of Port-Royal has the stern face of the ancient's Fatum; He withdraws into the clouds above, and comes out only to save from perdition a miserable creature. In Augustin the attitude is tender, trusting, filial, and if one is harassed, one feels the thrill of an unconquerable hope. Instead of crushing man under the iron hand of the judge, he allows him to feel the hand of the father who has prepared everything long before for the feeble child who is coming. "O Lord, the comforts of Thy Pity received me, as I have heard from; and so the comforts of woman's milk was ready for me. For my mother and my nurses did not fill their own bosoms, but Thou, O Lord, by their means gavest me the food of infancy according to Thy ordinance." Then his heart melts, thus thinking of the maternal milk. The great doctor gives to his style the tone of simplicity, familiarity, and humility, in order to tell us of his first wailings, of his outbursts of anger and of his delights in the days of his infancy. He was a father himself, he knew because he had seen with his own eyes, how touching is a newly born child, and the mother who offers her breast . . ."

Nothing could appeal more to us, to us who believe that kindness and charity are the real remedies for the painful social problems which impose themselves on our attention from all sides, than the words so full of human tenderness uttered by the great saint, words of adoration for the divine mother and for the celestial child, words which remind us that even for the formidable champion of God who inspired fear and trembling to his pagan enemies, religion means love.

Far from us be the idea of comparing Mr. Bertrand to Renan. But to bring home more forcibly the great change undergone in French thought, one may be allowed to recall that the literature which obtained with a former generation was reflected in the " Prayer on the Acropolis " of the great agnostic Renan, while to-day the man who pleases us is one who ends his book full of religious meditation with these beautiful words:

> "And now, whatever this book may be worth, which has been planned and written in a spirit of veneration and love for the Saint, the great heart, the great intellect named Augustin, for this unique type of the Christian, for the most entire and the most admirable servant of God who has perhaps ever lived, the author can only repeat, full of humility, what the first biographer, the Bishop of Guelma said fifteen hundred years ago: " I beg most earnestly from the charity of those who read this book to unite with me in thanksgiving and blessing towards the Lord who has inspired me to make known this life to those present and to those absent . . . and who has given me strength to achieve it. Pray for me and with me that I may endeavor to follow in the steps of that peerless man, in whose company God has allowed me to live for such a long time."

* * *

We have devoted special attention to Bertrand's book because he has faced so squarely the issue: Which inspiration do we want in our literature and in our philosophy? that of a Renan and an Anatole France, namely simply a great keenness of intellect, or that of a Saint Augustin, who to intellectual keenness would add above all the sense of responsibility of a writer towards the reading public, satisfy the soul as well as purely human reason? But, if not stated in such unmistakable and absolute terms, we can well say that the same deep anxiety is back of all the really significant books in France these four or five years past.

Let us mention very rapidly, and without entering into as many details as we did for Bertrand, some of such books as seem to us to express best this new trend of thought. And to do this we need not at all stop to consider writers who have for years proclaimed the advisability of a return to Christian, or better to Catholic, ethics, René Bazin, author of *Le Blé qui Lève* (translated), or Paul Bourget, who did so in all his novels from *Le Disciple* to the long novel published on the eve of the war, *Le Démon de Midi*, and since the war broke out in his novel, *Le Sens de la Mort* (1915). There are enough significant cases among those who (almost always abruptly) stepped forward to confess a change of views.

Surely one of the most interesting is Pierre Loti, who for so long had made use of his enchanted pen to lure us to despair; this man as he writes up his last pilgrimage to the land of Buddhistic resignation, pessimism and nihilism, inserts—they are the two last pages of his *Pèlerin d'Angkor* (1911, translated under the title of *Siam*)—this idea: And yet, I wish all this be not true: would it not be beautiful if, after all, *Christ* was right! This is not much, but as much as a veteran of the pen who has advocated all his life views so different can afford to offer. For the world at large, this page written by a Pierre Loti assumes enormous importance.

What we might call the most "sensational" case is that of Madame Juliette Adam, the remarkable woman who was quite a prominent figure during the Second Empire and the Third Republic, for years editor of the *Nouvelle Revue*, and whose seven volumes of *Memoirs* have been eagerly read on both sides of the ocean. Over thirty years ago (in 1883) Madame Adam had published a very militant novel of an entirely non-Catholic inspiration; boldly, in a decided tone of challenge, she had titled it *Païenne;* now in 1913 Madame Adam published another novel but which this time is called *Chrétienne*—which is given as a continuation of the first and gives an account of the conversion of the heroine from paganism to Catholicism, nay to militant Catholicism. Moreover the heroine in both novels is the authoress herself; they are not all the adventures of her exterior life which are told, but the adventures of her inner self—indeed she herself makes no secret about it. Surely one may differ as to the propriety of putting thus before the public one's own intimate self and doing it in such a dramatic way too; but the sincerity of Madame Adam cannot be doubted, and as she has always had in her the soul of an apostle, she may have considered it her duty to come forward as she has, in *Chrétienne,* in order to undo the harm she might have done with *Païenne.*

The third case we ought to record is that of Maurice Barrès—he is younger than the former writer but his remarkable mastery of style earned for him a very early fame, and since the war began he has had no equal in keeping up the spirits of his countrymen by the miracles of his pen. When he was a young man he was what is called nowadays a free-lance in the domain of thought. His incisive prose lent a real fascination to his irreductible radicalism; but his keenness of intellect which allowed him to uncover at once interested aims under the most beautifully presented theories, made it appear unthinkable that he would ever be anything but a destructive genius. But this man too yielded to some unknown pressure of events or of opinion, and in 1913 he published a long novel, *La Colline Inspirée,* in which he bows before the formidable and mysterious inspiration from things beyond human comprehension—an inspiration which leads astray, at times, naïve and helpless souls, but remains sacred even then as long as it is sincere; moreover—and this shows how Barrès also not only speaks of religion, but of *orthodox Catholicism*—the Church is there to prevent men from going astray, for they get lost only if they try to walk by themselves. M. Barrès is from Lorraine: his book is the story of a religious schism that took place about forty years ago in this province where deep religiosity is traditional; Joan of Arc came from Lorraine. The leaders of the sect who *are* sincere adorers of God, take for divine inspiration, suggestions which are far from divine. In fact the hero of the story is a religious crank, and the sacred spark is veiled by gross and vulgar behavior; the book is very painful to read; but the attitude of the author is remarkable. Ten or twenty years ago the story (it is a real story, as said above) would have been used as a magnificent occasion to discredit religion; now in 1913—and by the sharp ironist Barrès too—it serves to defend religion. How is it that the story of a crank, entirely lacking in intelligence of the world, filled with a conceit of the most ridiculous sort, still can interest us? Because in this man there is one thing that is stronger than any amount of crudeness of intellect, of conceit, or absurdity—genuine aspiration towards the infinite. M. Barrès also wrote that book—which is an act even more than a book—*La Grande Pitié des Eglises de France;* all through it from the first page to the last, we are moved by his passionate pleading for the maintenance and the restoration of the old churches and monuments which express so well the real inspiration of the history of France.

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One may ask with some pertinence perhaps how we explain, if the dispositions are so different now in his country, the last novel of Anatole France, who never wrote anything so bitter and violent against Christianity as *La Révolte des Anges* (1913). The answer is that we have in that senile product the natural outcome of all the philosophy of which he is a representative. And a man like Anatole France could not very well, at the end of a long career whose extraordinary success rested entirely on his talent as an ironical intellectualist, pass to the other side without risking the sensation of acting from feeble-mindedness and old age. In a way it is a cry of helpless rage. Moreover is it not something worth notice that A. France himself has not escaped the general dominating thought, and at the time when the world was so full of topics of all kinds, he too should have picked out *religion;* after this, let him answer a timid and cautious *yes,* or a ferocious *no*—this is of secondary importance; one thing is sure, the answer does not betray indifference; on the contrary, since the grimace of hatred is only another way of betraying one's concern.

* * *

The preceding authors (except Anatole France) had, only at the end of a long career, said *yes* where they had long said *no.* Let us come to the generation of authors who are now in full power of their talents.

Some represent a transition spirit rejecting the former agnostic attitude of most thinkers but never venturing any positive adhesion to Christianity or Catholicism. André Gide's *La Porte Étroite* (1909) is a good example of that merely negative thought, or Lafon's *L'Elève Gilles,* a novel which was awarded in 1912 the *Grand Prix de Littérature* by the French Academy (translated in English). The author died on the battlefield. The best known of this group, however, is Romain Rolland, the author of *Jean-Christophe;* he attacks the conceit of men of science, and the shallow hunt for something new at any cost concealed under the name of estheticism, and again German industrialism and militarism which dries up the soul, but he is always very cautious and timid: what does Jean Christophe want? *true* art:—what is *true* art? who does *not* want true art? and *true* philosophy? and *true* religion? What is there true or real or positive in the music dreamed and realized by Christophe?

Quite striking are two cases of men who go further than mere hesitation. The brothers J. and J. Tharauld became famous a few years ago by their novel, *Dingley, l'Illustre Ecrivain* (who was Kipling), a novel crowned by the Académie Goncourt. Then they tried various other fields in the note of realism. In 1913 they struck at last the new religious note and produced one of the remarkable books of these years, *La Tragédie de Ravaillac*—i. e., the story of the religious fanatic and insane man who killed Henri IV, the king of France. The idea that prompted them was exactly the same as Barrès's *Colline Inspirée;* and they wrote at the same hour, and they did not know of his book being written, as he did not know of theirs; but the same social or moral causes led to the same result. In a striking page they themselves explain how they were moved to write, namely, because they had still perceived in the madman, although crushed under mountains of fanaticism and superstition and insanity, the divine spark of Catholic faith which stirs all human hearts with a sacred emotion: and this mere spark was beautiful enough to warrant a whole book.

The other case is that of Binet-Valmer, probably a physician who achieved some fame in literature by discussing very freely some wretched types of modern society, perverted types most of the time. He was writing out of mere curiosity, as a dilettante thinker rather amused by the tragedies of his fellowmen. In 1913 (still this year *1913!*) he wrote a new novel which betrayed an entirely new man, *La Créature.* A very famous physician, psychiatrist, Dr. Batchano, has been entrusted with an unusually interesting case: a girl who, owing to a neglected education, has developed in her only the physical and animal sides of her nature; nothing human appeals to her; she is all instincts. He succeeds after tremendous care in raising her to the level of human intelligence. But when the work is all done—he realizes (he is forced to realize in watching the behavior of the girl in the world) that his cure is a failure. And gradually he comes to realize too that what she lacks: what he did not give her and what would have made her really human, are the two ideas of Duty and of God. This would be rather commonplace coming from the pen of some other writer: coming from Binet-Valmer, this is a hundred times significant.

* * *

These two novels are sporadic products in the career of their authors. Let us now mention three famous authors who show the Christian and Catholic spirit in their whole work—at least so early in their career that any previous product can be passed over.

One is the lyric poet Francis Jammes, now between forty and fifty years of age, who was awarded in 1912 the *Grand Prix de Poésie* by the French Academy for his *Géorgiques Chrétiennes,* indisputably the poet of France catering at the present hour to the largest public. He is a son of the sunny south of France, and his books tell his happy enjoyment of the nature of God, of his unsophisticated sharing in the sacraments and rites of the Church, after the years of youth when he was in the darkness.

Here is the Preface of the *Géorgiques Chrétiennes:*

> "I repeat once more at the beginning of this new book that I am a Roman Catholic, accepting very humbly all the decisions of my pope S. S. Pius X who speaks in the name of the true God; and that I am no adherer to any schismatic idea, modernism, or other; and that under no protext shall I deviate from the most uncompromising and from the most beloved dogma, the Roman Catholic dogma. The Roman Catholic dogma is truth itself coming from the very mouth of our Lord Jesus Christ through the medium of his Church. I repudiate in advance any claim on this poem that might be made by ideologs, by philosophers, by reformers."

He has emphasized so much, with the humility of a child, the abdication of his reason in the hands of the Church that some have accused him of pose and mannerism. Suppose there should be some affectation in the expression of his thoughts, which undoubtedly are a shock to our ears still accustomed to the agnostic refrains of a few years ago, why doubt his sincerity? Does not the soldier boast sometimes about his daring deeds—which he has performed none the less? Or does not the scholar often overestimate achievements which even though they do not revolutionize the whole world of thought may be none the less deserving real praise?

The second man is perhaps one of the greatest figures of French literature to-day. People in France have just begun to understand him. Appreciated only by a small élite for some years, he had just scored his second powerful success with the public at large in July, 1914,—the war broke out in August. The acting of his most characteristic play—he is chiefly a playwright—was the theatrical event of the season in Paris, 1913, and it is called—with perfect propriety,—owing to his religious inspiration, "a mystery"; the title is *L'Annonce faite à Marie.* Paul Claudel is in the diplomatic service of France. He started his career in New York and Boston. At the opening of the war he was in Hamburg. His first play (an American play) *L'Echange* is very realistic. But he soon turned to the deep note struck in those years by the author of *Saint Augustin.* The idea of *L'Annonce faite* *à Marie* is a revival of the ideal of the saints of the early Church. The way man bears suffering is the touchstone of his mental superiority—or inferiority; he who is visited by severe trial is he who is evidently considered by God worthy of it, is he who will manifest by his fortitude the beauty of a celestial soul; thus a real child of God rejoices in the severest trials because it gives him a chance to manifest divine loftiness in his actions. Violaine the gentle heroine who pays dearly for an act of pure love to a fellow man and who accepts in quiet exaltation the horrid existence of a leper, and whose burning charity is the medium of at least one miraculous deed, stands out indeed as a contrast to all our recent Christian women clamoring for rights to material happiness. One episode explains particularly well the Catholic ideal of Claudel. The scene is laid in France, terribly devastated by the Hundred Years War, and awaiting the liberator, Joan of Arc. The father of Violaine has been remarkably spared; his farm has not been destroyed or pillaged, his family is happy; and *for this very reason* he is filled with uneasiness. How is it that God has not tried *him?* Is he not worthy of it? Is it that he has involuntarily committed some grave sin that God will not give him the chance to show *his* fortitude? How will he appear before the Eternal Judge having no action of courage or resignation to claim which will allow him to take place among those worthy of entering the heavenly Kingdom? And he leaves all behind, wife, children, farm, starts on a pilgrimage to Jerusalem; surely he will suffer famine, thirst, sickness; in this way he can give the measure of his moral fortitude, and this will set his conscience at rest.

Another play, *L'Otage*, emphasizes even more this same philosophy.

The third man is the most original. We will just say two words about him, otherwise we would surely spoil the fascinating subject. Charles Péguy died a splendid death in the first days of the war, leading a charge at the Battle of the Marne. This has naturally added to his fame as a moral leader of the intellectual youth of France. In his early years he had followed socialism, but soon was disgusted with what looked to him simply a vulgar system of "sharing the plunder." What has made the greatness of France? He answers: the inspiration of the mediaeval faith, which for years and years we have stupidly studied only in its weak points. Like Claudel he sees the criterion of moral superiority in suffering for a good cause; *justice* he calls it; or devotion to humanity, to fellow men (—and of course the nearest fellow-men are those of one's own country); and in the crushing also of the unbounded pursuit of subjective pleasure which makes of men brutes, of women devils, and which results in social anarchy. Péguy gets his inspiration from the saintly men and women belonging to the age of the great French cathedrals, and who cultivated "the most beautiful garden of God on earth, France." He evokes with decided partiality these women saints, who finally become, as it were, the three chief deities of his most picturesque theology, *The Virgin Mary,* or *Notre-Dame, Sainte Geneviève,* the patron saint of Paris (who saved Paris from the Huns and Attila), and *Joan of Arc.* Why these three holy *women?* Evidently because women saints impersonate better the fundamental idea of charity, love, devotion. Joan of Arc, he specially honors, because he feels that France is on the eve of a great war, and the Maid of Orleans who fought valiantly was at the same time most kind to her enemies, and suffered extremely at the thought of the miseries of war.

Charles Péguy is difficult to read without preparation. His best known work is *Le Mystère de Jeanne d'Arc.* But probably his *Porche du Mystère de la Deuxième Vertu* (Cahier de la Quinzaine 22 Oct 1911) will be easier to understand,—or some of his verses, a selection of which appeared in 1914 (Ollendorff).

* * *

Now to end this somewhat dry enumeration, we will mention two representatives of the coming generation, so young that they could not have produced any really lasting work, yet who show all the more genuinely, since they are not yet masters of the pen, the inside of their souls. Their books are psychological documents rather than literary monuments. Both owe a part at least of their early fame to their family connections.[8]

One is the grandson of Pasteur, Robert Vallery-Radot. In his *Homme de Désir* we have no doubt many elements of a psychological autobiography. The hero tells of his education in very Christian surroundings, then how his studies in Paris made him to neglect his soul, then how after having measured the shallowness of human knowledge and art, he tries to get nearer divine life in the solitude of nature; but twice he is tempted by the deceit of terrestrial loves; after the fight that purifies, his soul finally finds celestial peace.

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[8] For instance, Mauriac, author of *Laure,* is fully the equal of Vallery-Radot in literary skill.

This book is nothing but a new edition, in the twentieth century, of the account of the Temptations of St. Anthony in the Desert; the scenery, the conditions are different, but the struggle against the low instincts conquered by a mystic devotion to God is the same.

Ernest Psichari is the grandson of Ernest Renan, and the religious fanaticism of the new generation as opposed to the dilettantism and scepticism of the great ancestor has been the subject of innumerable comments. Here, let us simply say that Psichari's first novel, *L'Appel des Armes,* has been the book of young unknown writers which has been most discussed and has been most widely read. Psichari tells of the lassitude that men of his age feel concerning mere theories and mere philosophical discussions, and how they want "action"; he chose to be a soldier. His determination to be a soldier is, however, interesting chiefly because he considers his new profession as a sacred mission. *War is divine*—the word is there. But he means that the soldier must be the representative of God's justice on earth, and fight all who use their power to crush the weak. His ideal is that of the Catholic middle-ages, when the Knight made a holy alliance with the Church to establish, even here below, a Kingdom of God. Let us mention the famous prayer which his hero offers in the Church of Cherburg before going to Morocco to fight the Infidels, as Roland and Charlemagne had gone to fight the Saracens:

> "O God give me courage and valiance . . . give me the faith of a soldier. God of the armies, if truly you are present in this consecrated wafer of the Communion, do me the favor of observing that I am not bad, and that I also am worthy to die for an idea. Send me to the distant countries of the Infidels, to the sunny battle-fields, and then give me the bravery of the old soldier. Make me strong, and permit that I may kill many enemies. . . . If you are willing, O Lord, grant me to die in a great victory, and then that I may see Your Splendor in Heaven!"

Psichari was a man of his word. Like Péguy, he died in the war, defending his cannons; he was a captain of artillery. He wrote an autobiography of his conversion which was published after his death under the title *Voyage du Centurion,* in the Christmas number of *L'Illustration* (prefaced by Paul Bourget). It is an enthusiastic and mystic product. There can be doubt whether the author wanted it published in that crude form. But it is a fascinating psychological document.

* * *

Just one word by way of conclusion.

The study of such men like Claudel and Péguy, like Vallery-Radot and Psichari, make us understand that the splendid spirit shown by the French youth in the war is no miracle, but is only a normal result of the thoughts and aspirations of the France which was in formation in 1914—a France which has decided to cast aside the enervating so-called artistic dilettantism, and old-fashioned naturalism. The names we have given by no means exhaust the list; they will suffice to show that we have not to deal only with sporadic attempts on the part of writers hoping to score success in sounding once more the religious note.[4]

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[4] This was the theory maintained by Salomon Reinach in the *Revue Sud Américaine* in May, 1914. He would probably write differently now, and no longer speak of "Snobbism." Moreover even if it were snobbism, it means something that the *snobbism* that will succeed is *religious* snobbism.

THE NUMBER OF OBSERVATIONS UPON WHICH A LIMEN MAY BE BASED

By E. G. Boring, Cornell University

Dr. Fernberger has recently discussed the "least number of determinations upon which the measurement of an individual's sensitivity can safely be based."[1] He concludes that with lifted weights "50 determinations is the smallest number upon which an accurate judgment can be based," and argues that the anthropometrist, who seeks to combine accuracy of measurement with economy of time, cannot afford to take less than 50 series. This limit is fixed by the curve of practice; the decrease in the interval of uncertainty when 25 series are increased to 50 is great; the further decrease in the interval when 100 series are taken is comparatively small.

The minimal number of cases upon which a limen can be reliably based must, however, depend upon something more than practice. Its determination must take into account the number of cases which will give statistical significance to the final average or limen. If this were not the case, Fernberger would do well to base his limen upon the second 25 series rather than upon the entire 50, and still better upon the last 10 of the 50, for practice would be maximal in the final series.

It is not possible, moreover, to place an arbitrary limit upon the number of observations; for the required number varies with the use that is to be made of the limen. A limen has scientific or anthropometric value only as it may be compared with other limens. A comparison, in terms of some such measure, of the savage with the American, of the right arm with the left, or of the morning with the afternoon, has in it the rudiments of a scientific conclusion. We are interested, therefore, not in limens, but in *differences between* limens. We want to be sure that the savage is more sensitive than the civilized man, or the right arm than the left. Even when the limen is determined for a practical purpose its comparison with some practical norm is still implied. And further we may lay down the general rule that the greater the difference we are seeking to establish, the fewer are the observations necessary to establish it. A dozen observations will demonstrate a difference in the limen of duality between the back and the finger-tip; five hundred may be necessary to show a difference between the two forearms.

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[1] S. W. Fernberger, The Effects of Practice in Its Initial Stages in Lifted Weight Experiments and Its Bearing upon Anthropometric Measurements, this Journal, 27, April, 1916, 261-272. See especially 270 ff.

Limens of dual discrimination (mm.) for the forearm (longitudinal) and the lower eyelid; differences between these limens; and probable correctness of the differences, arranged for various groupings of the 100 series taken in each case.

Series	Forearm			Eyelid			Difference		
	L	h	P.E._L	L	h	P.E._L	D	$\frac{D}{\text{P.E.}_D}$	Probable correctness
1st ten........	7.55	.040	1.96	5.70	.152	.456	1.85	0.90	.7270
2nd ten........	12.47	.083	1.15	5.76	.212	.403	6.71	5.52	.9999
3rd ten........	14.08	.046	1.60	5.29	.134	.473	8.79	5.26	.9998
4th ten........	7.61	.054	1.40	5.29	.166	.394	2.32	1.60	.8597
5th ten........	12.16	.087	1.30	6.81	.397	.255	5.35	4.05	.9968
6th ten........	14.00	.077	1.02	5.67	.207	.394	8.33	7.64	1.0000
7th ten........	12.74	.039	2.20	5.13	.324	.260	7.61	3.59	.9922
8th ten........	12.02	.113	1.11	5.59	.113	.580	6.43	5.14	.9997
9th ten........	9.66	.084	1.03	6.17	.248	.351	3.49	3.20	.9845
10th ten........	12.51	.110	0.84	5.49	.125	.570	6.80	7.68	1.0000
1st ten........	7.55	.083	1.15	5.70	.152	.403	1.85	0.90	.7269
1st twenty....	10.94	.061	0.91	5.58	.225	.225	5.36	5.73	.9999
1st fifty.......	10.28	.072	0.59	5.45	.221	.136	4.83	8.00	1.0000
1st hundred...	11.44	.081	0.33	5.70	.228	.095	5.74	7.67	1.0000

Let us take a concrete case. Suppose that we wish to show that the two-point limen upon the forearm is greater than that upon the eyelid. In the Table we find actual determinations of this limen (*L*) and of *h* in each of the two cases.[2]

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[2] The data are borrowed from the Cornell Laboratory and are for different O's. They are used here merely as an example of a method of treatment for data from a single O. The figures for the forearm were obtained in the quantitative drill-course by two exceptionally careful students, and are based on 100 series of 5 separations each. The figures for the eyelid are unpublished preliminary experiments by E. J. Gates on Observer W (*v.* this JOURNAL, 26 1915, 152 ff.), for whom 100 series of 9 separations each were used. Mr. G. J. Rich has kindly assisted me in the computations. Urban's tables were used.

The computations are made separately for each 10 of the 100 series, and also for the first 20, the first 50, and the entire 100 series. Our problem in each case is to determine the degree of significance which may be attached to the difference between the limens The computation for the first line of the Table is as follows:

For the forearm, $L = 7.55$ mm., $h = .040$. The probable error of the single observation can be found, for $P.\ E. = \frac{0.4769}{h} = 11.92$. The probable error of the limen, $P.\ E._{\text{L}}$ (corresponding statistically to the "probable error of the mean"), is $P.\ E.$ divided by the square root of the number of cases. The number of cases in 10 series of 5 separations is 50; but the 50 are not all equally weighted. A weighted value can be obtained by multiplying the sum of the values of P (Urban's tables) by the number of series, 10. In this case, the values of p are 40, 50, 60, 80, and 100%; of P, 0.98, 1.00, 0.98, 0.77, and 0 respectively. The sum of P is 3.73; thus the weighted number of cases is 37.3, and $P.\ E._{\text{L}} = \frac{11.92}{\sqrt{37.3}} = 1.96$. In a similar manner $P.\ E._{\text{L}}$ for the eyelid is found to be 0.456. The difference between the two limens is $7.55 - 5.70 = 1.85$ mm. The probable error of this difference is the square root of the sum of the squares of the probable errors of the limens, *i. e.*, $P.\ E._{\text{D}} = \sqrt{1.96^2 + .456^2} = 2.065$. The ratio of the difference to its probable error is $\frac{1.85}{2.065} = 0.90$. 8958 The value of the probability integral for this ratio may be found from a table of the integral to be 0.4539.[3] Thus there are 4,539 chances out of 10,000 that the difference will not deviate from the observed value by an amount greater than the observed value; or $10{,}000 - 4{,}539 = 5{,}461$ that it will so deviate. In only one-half of these 5,461 cases will the deviation be a decrease. Thus the probability that the difference is less than zero (deviates negatively by an amount greater than itself) is 0.2730. From this it follows that the probable correctness of the difference (*i. e.*, the probability that the limen for the forearm is greater than that of the eyelid) is $1.0000 - 0.2730 = 0.7270$.

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[3] There are simple tables arranged for this ratio; *v.* Table II in W. W. Johnson, *Theory of Errors and Method of Least Squares*, 1915; and the less complete table in E. L. Thorndike, *Theory of Mental and Social Measurements*, 1913, 200.

→ $\frac{D}{\sigma_D} = \frac{1.85}{3.0616} = .604$ (x in K-W tab

p = .727

Inspection of the column of probable correctnesses in the table shows that in many cases a difference based on only ten series may be highly significant. In the cases of the 6th and 10th series the probable correctness is 'certainty' within the limits of precision of a 4-place table. (In such a table any difference greater than 6 times its probable error is 'certain.') On the other hand, the low probable correctness of the differences for the first and fourth tens would doubtless render these series by themselves unacceptable as a basis for scientific conclusion. It should be kept in mind that 0.5000 is pure chance and is the minimal value obtainable. Thus a probable correctness of 0.7270 has no more significance than, say, a correlation of 0.45.

? 05±

The last four figures of the last column show that there was relatively little advantage in our continuing these experiments beyond 20 series in order to demonstrate a difference between the two limens. Had the difference been less, more series might have been necessary; had it been greater, 5 or 10 series might have sufficed.

The importance of practice lies not in its effect upon the magnitude of the limen, but in its effect upon constancy of judgment. The reason that the practised limen is a better anthropometric measure than the unpractised limen is not that it is smaller, but that it is less subject to variation.[4] This decrease of variability with practice is evident in Fernberger's *h*'s and in the *M. V.* 's among his observers. Lack of practice gives a small *h* and consequently a small probable correctness.

Any attempt to lay down rules for work upon the unpractised observer should be based upon a determination of the best number of practice-series as well as the best number of principal series. Fernberger seems to say that it is better to take 50 series than 25 because the interval of uncertainty is much nearer its practised limit in the former case than in the latter (7.90 for 25, 6.89 for 50); but from such an argument one might infer that it would be still better to take the second 25, and discard the first 25 as practice-series, since the interval of uncertainty for the second 25 is less than for the first 50 (5.96 for the second 25). Doubtless Fernberger does not suggest this possibility because he believes that 25 series are too few; but the reasonableness of this contention shows exactly how great is the necessity for always taking the number of cases explicitly into account. It would be interesting in Fernberger's experiment to be able to compare not only the second 25 with the first 50, but also the last 30 and 40 of the 50 with the entire 50, and the last 50 and 75 of the 100 with the entire 100. In this way one might, in any particular case of comparison, determine the total number of observations desirable and the number which should be discarded as practice-series.

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[4] Fernberger demonstrates this fact in his paper On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli, *Psychol. Rev. Monog.* No. 61, 1912, 19 ff. Doubtless he means to imply it in the place under discussion.

Even when practice fails to enter in at all, the problem of the least number of cases reliable remains. It is often desirable repeatedly to measure the same limen for a single practised observer under varying conditions. One may, for instance, wish to know how sensitivity varies with the time of day, with the degree of recovery from a pathological condition, or with casually induced organic or emotional states. Under such conditions the time available for the determination is likely to be limited, and the question then arises: can enough observations be made to give a satisfactory measure? Such a question can be answered only for the particular instance, since the greater the difference in question, the less the necessary number of cases. The greater constancy of the practised observer, however, makes it probable that we could accept fewer cases than would be allowable were we measuring individual differences between unpractised observers.

THE GESTURE OF AFFIRMATION AMONG THE ARABS[1]

By S. S. George

Modern theories of language assert that, at the very beginning, speech was gesture; the essential thing about primordial speech is not the sound but the movement. It is sometimes urged, against such theories, that different peoples use opposite gestures to express the same meaning. It is thus remarked that the modern Arabs, in affirmation, shake the head as we ordinarily do in negation. Wundt[2] quotes this observation, and tries to account for the contradiction by suggesting that the gesture may have some relatively recent and unknown origin. Goldziher,[3] on the other hand, cites discrepancies of this sort as proof of the lack of any necessary inner connection between meaning and gesture.

Wundt gives Goldziher as his authority; and Goldziher, in turn, refers to Petermann. The latter[4] is apparently responsible for the observation in question. It is my purpose here to examine the gestures of affirmation and negation among the Arabs, and to ascertain whether Petermann's observation, and the consequent discrepancy, are true to fact.

The first thing to notice is that the Arabs, according to their early traditions, at the time of the prophet expressed affirmation exactly as we do at present. Negation was manifested by a lift of the head upward, and affirmation by a drop downward and forward. The following is an incident which Goldziher also cites: "Asma, the daughter of Abu Bekr, relates, 'I came once to Aisha, on an occasion of a solar eclipse, and as she was in her prayers I spoke to her: "What makes the people so disturbed and terrified?" She pointed to the sky above. "Is this a true sign of God?" I asked her. She made a movement with her head (harraka) affirming.'" But while in this case we are not told what the movement was, the following anecdote gives us definite information. "There went on the streets of Medina once a maid, ornamented with earrings. A Jew threw a stone and struck the maiden dead. As she was still able to breathe, they brought her to the Prophet. The Prophet spoke to her: 'Did so-and-so kill you?' She lifted her head up (signifying a negation). The Prophet repeated the question with reference to some other person. She made the same movement (of denial). With a third trial, however, she moved her head downward (*shafadat*, i. e., affirming). On the strength of this the Prophet passed a death-judgment on the man concerned."

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[1] From the Psychological Seminary of Cornell University.

[2] W. Wundt, *Völkerpsychologie,* I, 1st part, 1904, 180.

[3] I. Goldziher, Zeitschr. für Völkerpsychol., xvi., 1885-6, 377.

[4] H. Petermann, *Reisen im Orient,* I, 1860, 172.

The etymology of the word 'negation' in Arabic is indicative in this connection. Negation or 'selb,' according to Muheet al Muheet, originally meant taking a thing (snatching it) unawares and with rapidity. Also to 'salaba' the sword, from the same root, means to unsheathe it; this is strongly suggestive of a movement upward. Similar correspondences are apparent in the case of 'affirmation.' 'Ijab' or affirmation is from the same root as 'to bring forth' (forward). The word 'kafada,' to bend forward, is used in two opposite senses. The Arabs say "the days have come down with me from a high place to a bent or low place (*kafd*)". They also say 'aysh kafd,' 'bent life,' to express a life of ease and luxury, in the opposite sense. Albustani, a great Arabian lexicographer who lived in the past century, commenting on this usage, says: "This is here used in the sense of acceptable life, because 'acceptable' (*merdieh*) is of the same meaning as *kafada* or bend forward." As occurs often in the Arabian Nights, the usual way of answering the command of a superior is by exclaiming, "Sam'an wa ta'at," which is equivalent to "to hear is to obey." (*Ta'at* is really more than to obey; it is also 'to do homage to,' usually expressed by a bending forward of the whole body, not only of the head.) These examples show that the Arabian traditions as regards the expression of affirmation and negation are fully in line with the procedure of other nations, and that their language is interwoven with references to such expression, whether in an etymological or in a purely idiomatic sense.

Did the modern Arabs forget these traditions and change the gesture of affirmation? To one acquainted with Mohammedan traditions and the tenacity with which they are adhered to such a change must seem extremely improbable. The literature of the modern Arabs, until very recently, is in great measure the old classical literature. Modern Arabs who read at all, if they do not themselves use the gestures in the same way as of old, must at least understand their import. To confirm these suspicions, however, I wrote to two authorities on modern Arabian usage, Professor R. Gottheil and Dr. Philip Hitti, both of the Semitic Department of Columbia University. Dr. Hitti is himself a Syrian scholar, formerly of Beirut; Professor Gottheil has travelled extensively in Syria, Palestine, and Egypt. Professor Gottheil writes: "I have never in my travels in Egypt, Palestine, and Syria, seen anything that would lead me to believe that the Arabs used the gestures in reverse order. I have asked Mrs. Gottheil, who is herself a native of Syria, and she tells me that the gestures there are exactly similar to our gestures in the West." Dr. Hitti confirms these statements.

As I have said before, the source of the alleged change of the affirmative gesture is Petermann's *Reisen im Orient*. Petermann does not dwell on the observation; he merely notes the fact in a passing remark. Remarks of this sort need not be taken very seriously, since Petermann is looking for 'incomparabilities' between Eastern and Western usages: a characteristic habit of the early travelers. What Petermann really observed was, probably, a very different thing from what he reported. I have myself observed among the modern Arabs sidewise movements of the head accompanying the expression of affirmation. The Arabic language is very susceptible to rhythmical intonation; and the Arabs indulge in sidewise harmonic motions on occasions which might at first seem incompatible, and to a Westerner quite incomprehensible. With their intonation in reading the Koran, the Mohammedans make slight movements of the body either forward and backward or from side to side. Again, such sidewise movements of the head or body are made on occasions of 'tarab' or pleasure-feeling. The Arabs say, 'ihtazza taraban,' shook with pleasure; or more often 'tamayela taraban,' rather in the sense of a slow and regular movement from the one side to the other. 'Tarab' itself in its origin means 'movement,' and also a movement of a rhythmical character (*cf.* Lisan Al-Arab, II, 45, 46). Other occasions when the Arabs perform sidewise movements of body or head are the occasions of 'hamasa;' these are occasions of pride, of self-exaltation, the sort of feeling one gets when engaged in 'great doings.' *Hamasa* is allied to *tarab,* but differs from it in being more nearly related to action, either preparatory to great actions, or accompanying them. It is thus interwoven with a 'warming up' pride. When a hero challenges another on the battlefield, he often recites certain 'hamasat' which, besides tending to overawe his opponent, also have the effect of warming him up to the highest pitch for the forthcoming death struggle (*cf.* Romance of Antar or Alzeer for numerous instances). The same movements occur in 'nekwa,' something like 'rising to the occasion' when called upon to act, a manifestation of chivalry. Such movements may also be clearly observed in 'waeed,' or threatening, when the speaker is aware that the execution of the threat will not take place immediately.

All these cases, it should be noticed, are related to one another. They all center on feelings of activity, pride of execution, or set determination. It is, after all, not strange that movements of this kind should be made in expressing affirmation. Any of the feelings mentioned might be present when an Arab affirms, and he might shake his head or body accordingly. Yet these movements, it need hardly be pointed out, differ from those made when a negation is to be expressed. In the first place, they are not so violent as in negation; the whole body, too, tends to be involved in the movement. The gesture of negation is accomplished by a more or less restricted movement of the face, the neck tending to remain stationary; in these other movements it is the head that moves, and the head and the neck are thrown alternately now to the one side, now to the other.

It is thus probable that what Petermann observed, and supposed to be a gesture of affirmation, was nothing more than a gesture expressing some such feeling as determination, pride of execution, or rising to the occasion. A closer observer, or one better acquainted with Arabian usages, would not think of connecting such movements with affirmation in itself, though they might well be made along with affirmation. Petermann's observation thus seems to be one of the many instances where a superficial and unscientific observation forces itself upon scientific men, and causes them not a little confusion.

POSITIVE AFTER-IMAGES OF LONG DURATION

By P. F. Swindle, Ohio State University, Columbus

A. Successive Color Induction:

  I. Of an owl (Glaucidium whitelyi Sws.) 324
  II. Of a cockatoo (Cacatua gymnopsis Scl.) 328
  III. Of human beings 329
B. Simultaneous Color Induction (of human beings)............ 331

A. Successive Color Induction

I. *Of an owl* (*Glaucidium whitelyi Sws.*). During the day the *owl* generally slept on its perch, which was a bar of wood passing through its cage at about five inches from the floor. In the night the bird frequently sprang from the perch onto the tin floor of the cage. It usually walked each time for many minutes to and fro on the tin floor, but it always hopped quickly onto its perch where it remained for some time, at least many seconds and often a few minutes, whenever I made an unusually loud noise in the room. The owl could fly, but it nevertheless always hopped, as is characteristic of many small owls, onto its perch. I wish here to emphasize the fact that the owl was never observed to miss its perch when it sprang at it, and it was moreover never observed to spring from one place to another on the floor; it walked on the floor and sprang off and onto the perch.

The following experiments with the owl were performed always at night in a room which was so dark that a human being could barely perceive white objects after long adaptation to the darkness.

As the owl stood, relative to my position, to the right of its perch and with its breast and head turned toward me, I turned on the electric light. The animal remained motionless on the floor and fixated my face until I beat a table with a ruler thereby making a loud noise. As usual it then sprang onto its perch. I turned out the light and waited until it was again in its previous position on the floor, and this time I turned the light as rapidly as possible on and off while the owl stood quite motionless and fixated my face. After I had thus illuminated the bird six or seven times I beat against the table with the ruler. I distinctly heard the owl leave the floor and then fall on the tin; the owl hopped, but this time it missed its perch. I turned the light on and found the animal still on the floor, but this time its back was turned toward me. In order to fixate me now it turned only its head almost 180° and to the left, i. e., anti-clockwise. While it stood in this position and fixated my face I turned the light repeatedly on and off in the tempo of about 0.5 sec. Immediately after the sixth illumination I beat against the table with the ruler. I again heard the owl hop on the tin floor and when I turned on the light I found it had really missed its perch and had indeed sprung in just the opposite direction; it sprang away from and came to rest with its back turned to the perch. Its right side was now turned to me. The two hops served to turn the bird clockwise almost 270°. In order now to fixate me, the owl turned only its head not to the left as before, which would this time have meant through almost 270°, but to the right, i. e., through an angle of a little more than 90°. I illuminated it as usual and after the sixth illumination I beat the table with the ruler. This time, however, the owl did not spring, but, as I could distinctly hear, it walked about rapidly on the tin floor. When I turned on the light I found the animal walking about excitedly to and fro and acting as if it were trying to get through the bars of its cage. Shortly it hopped onto its perch. Under the same conditions the thus far described reactions were, with only very insignificant variations, repeatedly obtained.

The cage was next turned so that the owl could be illuminated while it stood to the left of the perch. I illuminated it and then scared it just as before, and its reactions were the same as the previously obtained ones, with the single exception, however, that the two hops always served to turn the animal in the direction opposed to that of the hand of a watch.

The cage was then turned so that the perch was directly behind the owl; as the owl fixated me, no part of the perch was in its field of vision. As usual I illuminated it six times and then beat the table, and *without exception* the owl sprang onto its perch.

To obtain still another variation, I waited until the bird chanced to stand on the floor at the back part of the cage when I could see its eyes either below or above its perch. I illuminated it as before and made the noise with the ruler. Very often it succeeded in hopping onto its perch, b
it sprang too high and fell on the tin floor
the cage and at other times it apparently
perch and of course fell back onto the f

The above experiments were repea

modifications in so far as the number, duration, or tempo of the illuminations was varied. It seems to be the case that six illuminations were as good as ten or twenty. It became furthermore evident that it was not at all essential to beat the table immediately after the sixth illumination; for when I waited 25 sec. to beat, the owl also missed its perch regularly and indeed even after a lapse of 40 sec. the erroneous response was sometimes made.

When the owl made the first mishop I supposed the light had blinded it to such an extent that it could not see the perch, but this assumption proved to be of no value; for without exception when the owl could not see the perch while being illuminated, it made no erroneous hops. Only when a portion of the perch was in the owl's field of vision did mishops occur.

In order to explain these peculiar reactions I made the assumption that at the time I beat with the ruler the owl had a distinct after-image of the perch, which it responded to more strongly than to the real perch, i. e., the attempt was to hop onto the after-image instead of the perch itself. If this assumption deserves to be entertained we must suppose that the owl had, as a visual after-effect, something much more definite than a mere after-sensation; it must have been an unusually distinct after-image, which, with respect to form and color, was so similar to the illuminated perch that the owl might easily mistake this after-image for the perch. Otherwise expressed the after-effect must have been a strong positive after-image which of course wandered with the eye movements.

For those who have made no special study of owls, it may be here necessary to make the following explanation: Before an owl flies or hops (when excited some small owls hop) away from an enemy it faces, it usually first turns its head to the right or to the left as though it were looking for a new place to go. It may turn its head in the one and then in the other direction many times before it suddenly turns also its body as it flies away. Whenever the enemy remains unnoticed until it is very near, or, when a noise near the owl is very loud, the head is first turned, but it is nevertheless often *still* turning when the owl hops or flies away.

We are perhaps now in position to understand the described reactions of our owl:

Case 1. While the breast and face of the owl were directed toward me as I stood to the right of the perch, a part of this perch fell in the bird's field of vision. After I ceased illuminating, this part and the positive after-image of it were coincident until the owl turned its head in the direction of the combination of perch and after-image, then the latter was shifted to the right and since it was a peripheral after-image it continued to shift and was still shifting when the owl reacted to it by hopping. The attempt to hop onto the shifting positive after-image resulted in the bird's turning through an angle of almost 180° in the direction of the hand of a watch, and of course in its falling on the floor. We must not forget that the owl did not stand long and watch the after-image shift as the head turned; it hopped while the head was still turning.

Case 2. The owl then stood with its back and face turned to me. I illuminated it and then beat the table. Preparatory to the springing the head was first turned in the direction of the perch, but the tendency to follow the shifting after-image caused the head to finally move through the large angle of about 270°. Since the body was already turned almost 180°, it was this time easier for the bird to turn the head farther than before and thus cause the after-image to shift about 90° farther to the right than in Case 1. The animal tried to hop onto the shifting after-image and fell.

Case 3. The owl stood to the right of the perch with its right side turned to me, and, in order to fixate me, it turned its head a little more than 90° instead of almost 270° as it would have been compelled to do if it had moved to the left as before. When I this time beat the table the owl turned its head allowing the face to pass over the breast. The after-image was necessarily thrown through the bars and the owl did not often try to spring through these bars, which it could see even though it was dark; its response was that of walking to and fro as near as possible to the place where it last saw the after-image.*

Case 3a. This time the owl stood to the left of the bar and was illuminated. It made the same general responses as it did in the Cases 1, 2 and 3, but the first two hops served to turn the body in the direction *opposed* to that of the hand of a watch. When the owl turned its head in the direction of the combination of perch and after-image, the latter was this time necessarily shifted in the direction *opposed* to that of the hand of a watch; the owl followed this after-image when I beat the table the first two times. When I beat the third time, the conditions were analogous to those in Case 3, and the owl's response was consequently the same as before (Case 3); it walked to and fro along the bars.

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* The bird subjects mentioned in this paper were specimens from the Zoological Garden in Berlin, Germany, and were kindly furnished me for research purposes by Prof. Heck, the director of the garden. The experiments with them were performed in my private research rooms in Charlottenburg-Berlin, from July 1, 1914 to Sept. 1, 1915. The experiments with human beings were in most cases first roughly performed in the animal rooms and then more systematically in the psychological laboratory of the University of Berlin.

Case 4. The owl stood directly before the perch and fixated me; no part of the perch fell in the owl's field of vision. Since there was no occasion for a response to an after-image as before, the owl succeeded each time in hopping onto the real perch.

Case 5. The owl stood in the back part of the cage and had me and also the perch in its field of vision. When I beat the table the owl made few mishops, but it nevertheless sprang occasionally too high or too low. The perch and the after-image were evidently not always coincident as the bird left the floor; the after-image was thrown, owing to movements of raising or lowering the head immediately before the hop was taken, sometimes above and sometimes below the perch.

II. Of a cockatoo (*Cacatua gymnopsis Scl.*) Corresponding results were won from the cockatoo. While in artificial light, in daylight or in the twilight this expert climbing bird walked back and forth on its perch and never made a misstep. In the dark it walked, if at all, quite slowly and very little, as compared with what it did at other times, but even under the condition of darkness it never fell from its perch. It often walked without speaking, but as it talked it generally walked.

I placed an electric light just outside the cage and near one end of the perch, which was a rounded stick of wood attached to two sides of the cage. As the cockatoo stood on the distal end of the stick from the light I illuminated it as I did the owl. I illuminated it six times and then spoke to it; it began to speak just as in the daytime, but often before it had completed the first word it fell backwards from off its perch.—The cockatoo can see very little if at all in the dark; therefore when I spoke to it and it made the usual response of talking it of course started to walk to the other end of the perch, *i. e.*, in the direction of the extinguished light. Perhaps, however, it could not see the perch but only the positive after-image of it. When it started to walk it turned its head in the direction of the extinguished light and thereby caused the after-image to shift, and with respect to the bird's body, in the backward direction. It took a step to grasp this after-image, which was no longer coincident with the perch, and quite naturally lost its equilibrium and fell backwards. It made no difference which side of the cockatoo was illuminated; it always fell backwards when it stood, while being illuminated, on the distal end of the perch from the light. I often noticed that the bird, after it had lost its equilibrium, was still holding the perch with one foot; this was always the foot most distant from the light.

When the cockatoo stood on the end of the perch near the light and was illuminated as before, it never fell. It behaved in this case just as it usually did when I spoke to it in the dark. It spoke very slowly and also very little, and instead of walking about on its perch, it merely threw the weight of the body first on one foot and then on the other. The failure of the bird to respond as vigorously as in the previous experiment was due to the fact that the shadow of the bird's body fell on the perch; for when I left the bird in its position and placed the light at the other side of the cage, the erroneous step was again taken.

The missteps of the cockatoo did not occur as regularly as the mishops of the owl, but this is evidently due to the fact that the cockatoo did not remain as quiet as the owl while being illuminated. It was also the case that the after-images of the cockatoo did not persist as long as those of the owl; for when I spoke to it 25 sec. after the last illumination, it behaved as it usually did in total darkness. When I waited only 20 sec., however, it very often fell from its perch.

III. Of human beings. Although the conditions of the experiments were apparently just the same for me as for the birds, I was nevertheless unable to observe positive after-images of long duration. I took great care, however, to remain as motionless as the owl, while I illuminated myself (and of course the objects about me) just as I did the birds, and finally I observed a positive after-image of my hand, but this was of only two or three seconds' duration. But one time I waited about five seconds after the sixth illumination and then illuminated my hand only once. This time I could scarcely convince myself that the light was not still burning; it seemed as if I could see my real hand, the real objects on my table and even the pictures on the wall. These decidedly positive after-images persisted for many seconds, and when I turned my head, they were shifted just as I supposed was the case with the birds. Since this time, I have often observed these after-images for forty seconds and one determination with the stop-watch shows that one positive after-image of my hand persisted for 54.4 sec. (I tried the new method on the cockatoo and found that it more frequently made the missteps.)

Since I have had much practice in remaining still while being illuminated, I am at the present time able to observe very distinct positive after-images of long duration even though I illuminate myself only once, but a necessary condition for the observation is that I remain for a few minutes in the dark.

When I fixate with both eyes and then close them carefully after the last illumination, it is often difficult for me to believe that they are really closed. Observers often exclaim, " My eyes won't shut," " I've lost control of my eyelids," etc. This illusion is evidently due to the fact that the after-images, which have the same appearance, as far as color and form are concerned, as the illuminated objects, are not only just as distinct as when the eyes are open but usually much more so.—Another very peculiar illusion is to be noticed when the observer, while being illuminated, fixates with only one eye, e. g., with the right one, and then opens also the left eye. It often seems as if through some painless procedure the left eye were being pulled out of the head. Some of the observers related their experiences in the following ways: " I can't see with the eye that was closed," " I am blind in that eye," " It feels as though that eye were falling out," " I have a very peculiar sensation in that eye, but just what's happening to it I can't exactly say," etc.

It is to be noticed that when any part of the body makes a sudden movement, the positive after-image suddenly disappears. This is without doubt connected with the fact that the after-images observed while the eyes are closed are of longer duration than otherwise; for then such sudden involuntary movements of at least some parts of the visual apparatus, e. g., the eyelids, no longer take place.

After a positive after-image has disappeared, a negative after-effect is often to be observed, but the latter is relatively very indistinct, in so far as it is not characterized by having a definite form. I therefore prefer to speak of the negative after-effect which succeeds the positive after-image, as a negative *after-sensation*. According to my observation the after-sensation is generally not of such long duration as the after-image. It is often to be observed that as soon as the first negative after-sensation disappears a positive after-effect again appears, but this is very weak, indefinite as to form and of very short duration. This we may also call an after-sensation, and of course a positive after-sensation—We should theoretically conclude that if the observer should once be illuminated at the particular time when the negative after-sensation has run its course and the positive after-sensation is about to appear, the latter would be strengthened to such a degree that it would become converted into an unusually distinct and long lasting positive after-image This is in fact the case. If the positive after-image lasts, for example, 20 sec., then the experimenter, in order to obtain the most marked effect, should wait about 20 sec. longer and then illuminate the objects in the observer's field of vision once more.

Since it at least in some cases occurs that in successive induction a positive after-image and indeed one of long duration can be brought about as the first after-effect, we should for logical reasons conclude that a phenomenon of fundamentally the same nature must, under proper conditions, also come about in simultaneous induction. Experimental facts, which we shall next discuss, demonstrate beyond a doubt that this is really the case.

B. Simultaneous Color Induction (of human beings)

The material used for our main experiment is a piece of green paper (any color may be used) of about 4 x 4 inches and a gray background (or even a white one) of about 20 x 20 inches in the central region of which the green paper lies. The observer holds the eyes at a distance of about nine or ten inches from the green paper and carefully fixates a point on it; a point near the center is best. It is also for this experiment necessary for the observer to eliminate through practice many involuntary movements of the eyes and head.

The paper that is being fixated becomes less and less greenish, as is well known, but long before it appears reddish (i. e., provided the observer does not allow the eye-lashes to fall and partially obstruct the vision) the entire background becomes greenish. Green is the first color to be induced on the background by green. Following this first induced color, which is unmistakably green, appears an unmistakable red. The red does not appear first. Red is the last color to be induced when one carefully fixates green. When the red disappears, the background again becomes greenish.

If, when the first induced green on the background is on the verge of disappearing, the green paper is quickly remove to expose a red paper of the same size, which can be be hand glued to the background piece, the red on the background becomes in a short time by far more pronounced than if the green paper had remained. (This experiment is essentially the same as one we performed in successive induction, p. 331.

Since the induced color can have the form of the background, we may speak of it as a positive *after-image*. This expression has its advantages, since there are occasions also in simultaneous induction when it is of value to speak of positive and negative *after-sensations*. Some of these after-sensations would be the very small indefinitely bordered areas of color which are to be observed on the background after a period of very long fixation.

As is well known a red border develops around the green paper that is being fixated. If the eyes are held very still this border is narrow; if they are allowed to wander, without leaving the paper entirely, it may become very wide, and if the eyes wander to a distant point on the background, one can no longer speak of a border but of a red after-image of the *entire* green paper instead of only the outer edges of it. If in a dark room I illuminate the paper for a *very* short time, I can observe a positive after-image of it, and if I wait until the green after-image disappears, I see red; but it is not at all necessary for me to do just this in order to see the red. I can illuminate the paper for a greater length of time and thereby avoid having to notice the positive after-image; I observe red at once. Moreover I do not need to do just this. I can fixate the illuminated paper for a few seconds and then turn the eyes away from it while the light is still burning; also in this case I observe the red immediately. In view of these facts we must necessarily conclude not only that our red border is due to small involuntary eye movements, as is indeed often supposed to be the case, but also that the red of this border is by no means the first but the last color induced by the fixated green.

Voluntary as well as involuntary eye and body movements play a significant rôle in the case of the Hering window. This window, when recklessly used, is indeed a singular device to blind and deafen one to our general law of color induction which we may now formulate as follows: *In successive and simultaneous color induction, any color induces first itself and last of all its antagonistic color.*

When a green and a white window is used, and the two shadows of a board five inches in width are allowed to fall on a large screen of thin linen, one of these shadows appears of course green to an observer who stands behind the screen, and ordinarily the other shadow appears to be distinctly red. This judgment would seem to indicate that our law is at least for this case not valid. Fortunately, however, one can, while the windows are closed, fixate a point on the screen where the red shadow would ordinarily be observed, and continue careful fixation until the windows have been gradually opened. In this case the shadow does not appear reddish at first; it is at first merely a dark shadow. Careful observation shows that it becomes in a short time greenish and then reddish. If a point to one side of the shadow is fixated, this shadow becomes greenish still sooner.—These results can best be obtained if the observer sits at a distance of about three feet from and preferably behind the screen. The only advantage in his being behind the screen is that shadows of the observer's body do not also fall on the screen and thus complicate the conditions of the experiment.

Instead of having to open the windows the experimenter can cover them with a cardboard and at the proper time gradually remove this. If the board is suddenly removed, the induced red *generally* appears to the untrained observer at once; if the observer has carefully trained himself to keep the eyes still while he fixates, the red often does not appear suddenly. If the board is allowed to fall on the floor and thereby produce a distinct noise, the induced color *always* (at least always to the untrained observer) makes its appearance immediately. After the observer has heard this noise some few hundred times, it then makes no difference whether the cardboard is merely quickly removed or whether it is allowed to fall from the windows. This means nothing more than that the observer has been trained to make a negligible response to the noise.—If now the cardboard is gradually removed and the observer notices no induced color, he will, however, notice the red in the shadow immediately when the experimenter slaps his hands together, stamps with his foot or fires a gun.

These facts lead us to conclude that the sudden appearance of the induced negative color is usually connected with sudden muscular responses of the observer; these may be (so far as the just mentioned observations indicate) responses to sudden noises or to a sudden change in illumination.

In connection with these statements we must ask
the question why it should not then be the case th
observer fixates a hand that is illuminated f
time in the dark room, he does not likewis

change in illumination. I am convinced that he does; for his positive after-image of long duration is indeed analyzable into two positive after-images. The first one of these is so intense and of such short duration, really of such flash-like duration, that it cannot be very carefully observed. However short it is it can nevertheless usually be observed to wane; sometimes, however, it disappears quite suddenly. Only when this one wanes does the second positive after-image appear. The second one generally maintains a constant and too a remarkable degree of distinctness until just a very few seconds before it suddenly vanishes. If the observer moves quickly as soon as the light is extinguished, the first after-image disappears quite suddenly (it does not merely wane as before) and often the second positive after-image cannot be observed at all; instead of it the negative after-effect makes its appearance.

The first positive after-image has been discovered and is being used by the kinematographists. I am convinced that the second one has also been earlier observed and has long been and is still being used by spiritualists. One special fact which would render this after-image of particular interest to spiritualistically inclined persons may best be told by relating one of my own experiences with it. I once illuminated a person in the dark room and observed a distinct positive after-image of him. We then went into a well-illuminated room and talked for about forty minutes. I then went alone into the dark room, remained about ten minutes and then with *closed* eyes illuminated the room once more (a single illumination) and immediately I observed a distinct positive after-image of many seconds' duration of the person whom I had previously fixated. Several of my subjects have had analogous experiences.

A SIMPLE DAYLIGHT PHOTOMETER

By C. E. Ferree and Gertrude Rand, Bryn Mawr College

A need for a simple daylight photometer has long been felt, especially in the work of the undergraduate laboratory. The impossibility of making determinations of color sensitivity even with a degree of precision that is acceptable in undergraduate work without constancy of illumination especially when pigment papers are used as stimuli is too well known to need more than mention here. To make such an instrument broadly serviceable the following are some of the requirements which should be met. (a) The instrument should be compact and easily portable. (b) It should be so simple and inexpensive in construction as to be readily within the mechanical resources of the average laboratory. And (c) the standard and comparison fields should present little if any color difference.

An instrument which we have constructed especially to meet the above needs is shown in Fig. I. It has been in use in our laboratory for more than a year and has proven so serviceable and convenient that we have thought it worth describing for the possible benefit of others. It was designed and has been used by us primarily for the reproduction of a given intensity of illumination rather than for its measurement in photometric units, although it can be calibrated and be used for photometric measurements. The instrument consists of a photometer head, a short bar, a standard tungsten lamp with carriage which is moved back and forth along the bar by means of a rack and pinion, a millimeter scale which may be read outside of the photometer box, a finely graduated ammeter to regulate the supply of current to the lamp, and a tripod support. When operated as a daylight photometer one opening of the photometer head, the bar, and the standard lamp with its sliding carriage must be boxed in; and the other opening of the photometer head be suitably exposed to the illumination that is to be balanced against the light of the standard lamp. This boxing can be made as elaborate as one chooses or it can be made very simple. In this connection the different needs that may arise for a portable photometer should be kept in mind. One may want, for example, to determine the average illumination, or the distribution of light in a room which may or may not be evenly illuminated. To do this the room should be laid out in small squares and measurements be taken in several directions of horizontal, vertical and 45 degree components of illumination at the corners of these squares. For such work it is obvious that a somewhat elaborate photometer is required, comprising, for example, a test plate that can be turned in different directions and a type of boxing that will permit of a quick adjustment of the lamp and reading of the scale from the outside. Such a photometer we are required to employ for the specification of the lighting effects in our work on the effect of different conditions of artificial lighting on the eye. An instrument of this kind, however, may cost from one hundred and fifty to three hundred dollars, which is of course more than is justified for the work of the general laboratory. If, however, an instrument is wanted primarily to reproduce the horizontal component of illumination or the light falling on a vertical surface such as a campimeter screen, rotating disk, etc., at a given point in a room, a very simple boxing is all that is required; for all that is needed here is to set the standard light at such a position on the bar as will balance the light in the room at that point and keep it there as long as that intensity of light is wanted. We have found it quite sufficient in one instrument we are using to make this boxing of light-proof cloth sliding on a suitably constructed frame. This cloth may be folded back to the far end of the frame for the adjustment of the position of the lamp or it may be brought forward and hooked to the frame of the photometer head while the photometric balance is being made. In case of the instrument described in this paper a somewhat more elaborate but still simple boxing is used. This boxing is made of heavy sheet tin painted black outside and inside and carefully light-proofed. It is 18 inches long, 4.5 inches wide, and 10 inches deep.[1] The photometer head forms one end of this box; the other end is of fiber fitted with binding posts which connect with the line and with cords running to the standard lamp, and with a knife switch to make and break the circuit. The top of the box is covered with tightly fitting hinged lid which permits of a convenient and easy entrance to the box. Projecting through the side of the box is a milled head which operates the rack and pinion adjustment of the position of the standard lamp on the bar. The instrument with the boxing is shown in Fig. II.

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[1] It may seem that the boxing of this instrument is unnecessarily deep. It was made deep in order that lamps of ordinary sizes might be used as standards. The boxing shown in Fig. II is designed to take 25, 40, and 60-watt Mazda lamps and to allow for the adjustments of height needed to bring the centers of their filaments in line with the center of the opening of the photometer head. If smaller special lamps were used so much depth would not be needed and the instrument could be given a neater appearance.

The photometer bar is 24 inches long. At one end of this bar is a right-angled holder for the photometer head. The photometer head is supported on a brass rod 5 inches long which passes vertically through an opening in the right-angled holder. When adjusted to the height that is wanted it is held in position by means of a set screw. The carriage for the standard lamp is shown in Fig. I. This carriage is fitted also with a right-angled holder and set screw to hold the standard lamp and to provide for adjusting its height so that the center of the lamp may always be in line with the center of the adjacent opening in the photometer head. On the bottom of this carriage is fastened a rack 12 inches in length which is engaged by the pinion operated by the milled head already mentioned. To this carriage is also fastened a brass scale graduated in millimeters which extends through

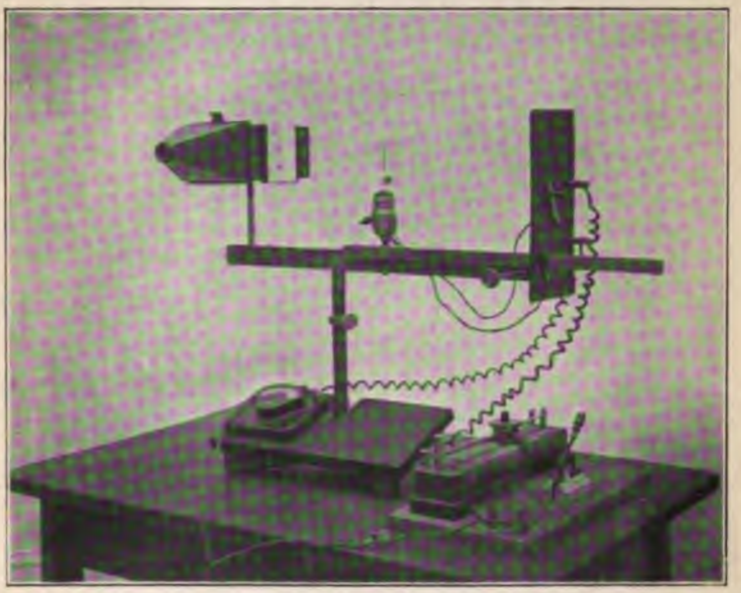

FIGURE I

an opening in the fiber plate forming the end of the photometer box opposite to the head. Thus as the lamp is run back and forth along the bar its position can be read, outside the box, from the divisions on the scale. To facilitate the reading of these divisions the scale runs immediately back of a short pointer fastened to the end of the photometer box.

The photometer head employed is of the Bunsen type. This type of head is especially suitable for our purpose because it combines to a favorable degree the features of accuracy and simplicity of construction. The photometer screen may be very simply made. In the present case it consists merely of two pieces of Hering mat white paper 12.5 cm. long and 8 cm. wide smoothly pasted together with the mat side out. The screen so formed can be overlaid if desired with magnesium oxide deposited from the burning metal. In the median line (horizontal) of this screen, 1.5 cm. from one end a circular opening, 1.5 cm. in diameter, with serrated margins, is cut. This opening may be filled with a layer of an extra good grade of tissue paper or other translucent material, the edges of which are held between the two layers of Hering paper. It is desirable to have a material to fill this opening whose

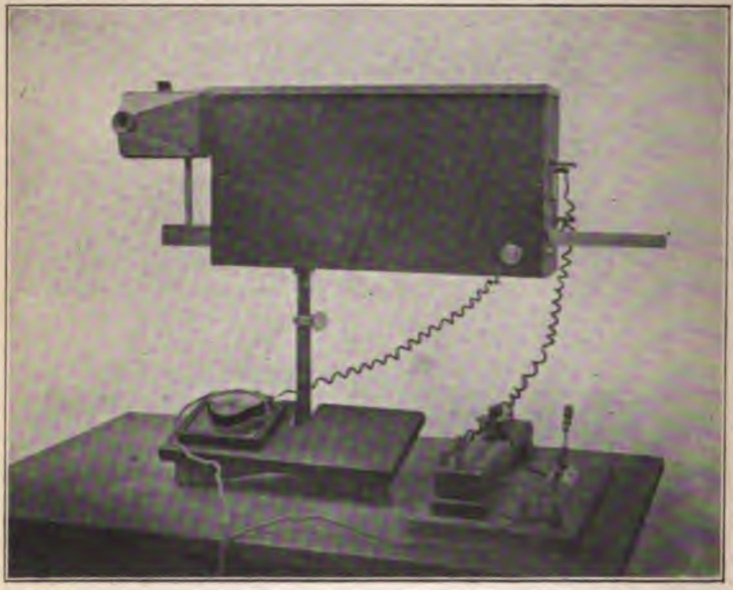

FIGURE II

coefficient of transmission is as nearly as possible equal to its coefficient of reflection. This screen fits into a groove which runs from front to back in the median plane of the photometer head. Set into the back of the photometer head on either side of the screen and making an angle of about 65 degrees with it are two mirrors of suitable size in which the images of the two sides of the screen are viewed by the eye in making the photometric comparison. On either side of the photometer head are two openings, 3.25 x 2.5 inches, for the illumination of the photometer screen. One of these admits the light from the standard lamp, the other the light from the room. Both of these openings are filled with a plate of single-thick milk glass (Belgian make) ground on one side. This glass diffuses the light and gives a more uniform illumination of the two sides of the photometer screen. In order that the two sides may be illuminated by light of the same color quality, color filters must be employed. That is, either the standard lamp must be robbed of its excess of yellow and red light or the daylight must be colored to match the light from the standard lamp. Either of these effects can be readily accomplished by means of thin sheets of colored gelatines, placed in the grooves in front of the sheets of milk glass. With gelatines of a low coefficient of selective absorption it is not at all difficult to make a good match of the two lights as to color quality and thus to eliminate the difficulty that attends the attempt to make a judgment of equality of brightness between two surfaces which differ as to color quality. In making this match by means of filters it must be remembered that if the match is made by filtering the daylight, a slight physical error will be introduced because of the variable composition of daylight on different days and at different times of the same day. That is, a filter that transmits heavily in the yellow will let a greater total of light through when the daylight contains an excess of yellow than when it does not. This objection, however, is considered by some photometrists to be of more theoretical than practical consequence. To offset this objection the greater photometric sensitivity to yellow may perhaps be mentioned with some justification. The variable composition of daylight also causes some difficulty in maintaining an exact color match between the standard light and daylight. A filter that produces an exact match at one time may not at another time. For this reason it is of advantage to make the filters of thin layers of gelatine which can be added to or subtracted from with the proper corrections for absorption as the need arises.

The bar carrying the standard lamp and the photometer head is supported by a tripod base and stem. The stem consists of a hollow tube split at the upper end and fitted with a collar and set screw. The stem telescopes over a rod 8 inches long which is screwed into the photometer bar 8.5 inches from the end supporting the photometer head. By means of the collar and set screw the apparatus may be adjusted and clamped at different heights.

In order that the standard lamp may be operated directly from the line a rheostat and finely graduated ammeter are used to regulate and keep constant the supply of current. For the sake of portability the ammeter is fastened to a wood base which is screwed to two of the feet of the tripod. The ammeter is of Weston make, triple range, 0.5, 1, and 1.5 amperes, combined in one case. The scale of the first of these ranges is graduated to 0.01 amperes. On account of its size, its graduations, and its comparative inexpensiveness, this ammeter is very well suited for the purpose. A specification of the rheostat need not be given here. Any good rheostat of suitable carrying capacity and range of adjustment which permits of fine changes of resistance may be used.

The use of the apparatus for the reproduction of any given illumination is as follows: The rheostat is adjusted to give the amperage at which the standard lamp is to be operated. A balance is then made at the point in the room in question between the light falling on the photometer head and the standard light, and a reading is taken of the photometer scale. When it is wished to reproduce this illumination the resistance is again adjusted to give the reading of the ammeter chosen as standard and the light of the room is varied until a photometric match is obtained. If it is wished to calibrate the instrument so that the reading of the scale can be translated into foot-candles, for example, a standard lamp is set up at such distance from the milk glass test plate on the photometer head as will give a balance with the photometer lamp adjusted for the different points on the scale. The amount of light falling on the test plate can be computed directly from the known flux of the standard lamp and the distance of the lamp from the test plate. This is correlated with the division on the scale for which the photometric balance is made. The different points on the scale are thus gone over one by one and the correlative foot-candle values are obtained. During the calibration the photometer lamp must of course be operated at a constant amperage, and in the use of the calibrated instrument this amperage must be reproduced else the calibrated values will not be valid.

THE NATURE OF RECOGNITION MEMORY AND OF THE LOCALIZATION OF RECOGNITIONS[1]

By MARGARET HART STRONG and EDWARD K. STRONG, JR.

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I. INTRODUCTION

Professor Woodworth[2] has said, " It is worth noting that the feeling of familiarity varies in *quality* as well as in intensity. A person may arouse in us a feeling of having been recently seen or of having been seen a long time ago, of having been seen under agreeable circumstances, or of having stood in some definite relations to us, as of co-operation, antagonism, etc. No experimental study has yet been made of these varieties of the feeling of familiarity." Wundt[3] has also maintained that each act of recognition " possesses its own peculiar quality." Lehmann[4] pointed out that recognition has two meanings in daily life: (1) to recognize, knowing you have seen previously, but not knowing where; and (2) to recognize, knowing you have seen previously and knowing where, when, etc

In this paper we have attempted to obtain a better conception of what is involved in those qualities which seemingly can be thought of as added to the apparently simple process of recognition, or, in Lehmann's words, to discover what is involved in the process of knowing where, when, etc., one has seen the object before, as distinguished from simply knowing that one has seen it.

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[1] Read in part at the Chicago meeting of the American Psychological Association, December, 1915.

[2] R. S. Woodworth. Mimeographed lecture notes on recogni memory, Summer School of 1913, p. 63.

[3] W. Wundt. *Introduction to Psychology*, p. 105-10

[4] A. Lehmann. Ueber Wiedererkennen, *Philos. St* 96-156.

We have attacked here specifically the problem of how one localizes the previous experience of an object, as well as how one recognizes that object. From our results we believe that localization may be due either to some affective process, or to a system of associational reactions which link the object with other objects whose time-relations are known. The latter type of localization is with us introspectively different from the former, and we are not concerned with its analysis here. The nature of the mental process in the affective type of localization is not very clear, as the result of this study; but we are inclined to believe that it is essentially a finer subjective analysis of the same consciousness that conditions recognition itself.

We present first our experimental results which throw some more light on the process of recognition, as well as on the process of localization itself; then we pass to a general discussion of the nature of recognition and finally to that of localization.

II. The Experiment

The same general procedure was followed here as in the experiments reported in our 1913 *Psychological Review* article.[5] In that investigation we used lists of 20 words which were read through at the subject's leisure, *i. e.,* "he was told that he might read them slowly enough to actually grasp the meaning or content of the word, but must read them fast enough not to be able consciously to form associations between them." After a certain interval of time a reaction list was presented containing the first 20 words and 20 new ones. The subject was asked to check the words he recognized as having been seen in the stimulus list. He indicated the certainty of his recognition by marking a "1" opposite words he was absolutely sure of, a "2" after words he was reasonably sure he had seen, a "3" after words he had a faint idea he had seen, and a "4" after pure guesses. A "4" was necessary because the subject was required to check 20 words each time. (For further details of the procedure, see article in question.)

The present investigation employed the above procedure and three other procedures, all variations of the first. In Experiment A the above procedure was used, a stimulus list being given on Sunday and the reaction list on Wednesday, four days later. On the same day the second stimulus list was given and the reaction list followed two days later on Friday. The third stimulus list was then given and its reaction list followed on Saturday. The fourth stimulus list was then given and its reaction list followed five minutes later. From Experiment A we can determine the effect of time-intervals of 4 days, 2 days, 1 day, and 5 minutes on recognition memory. These results can be compared with those obtained for these intervals of time in the 1913 *Psychological Review* article.

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[5] E. K. Strong, Jr. The Effect of Time-Interval upon Recognition Memory, *Psychol. Rev.,* XX., 1913, 339-372.

Experiment B was similar to Experiment A except that the stimulus lists were presented on Sunday, Tuesday, Wednesday and Thursday mornings, and then all four reaction-lists were reacted to at the same time.

Experiment C differed from Experiment B in that the entire 160 words were jumbled up into one long reaction list instead of four shorter lists. In Experiments A and B the subject knew that the reaction list contained 20 old and 20 new words and he knew just how long a time had elapsed since he had previously seen the old words. In Experiment C he knew that the reaction list contained 80 old and 80 new words, but he was not informed as to how long an interval of time had elapsed since he had seen any specific word.

Experiment D differed from Experiment C in that the reaction list contained only the 80 old words and no new words. There was no check here on the subject's recognitions and of course he knew that all the words in the list should be checked with a "1." But the experiment was run to see just how knowledge that all the words had been seen before would affect recognition.

Now besides the above phases of this investigation dealing with *recognition* there were two further parts dealing with *localization*. After the subject had marked the lists in Experiments C and D for his recognitions he was required to mark opposite each word when it had been previously seen and also just what degree of certainty he had of this localization.

The two writers acted as experimenter and subject for each other. Five complete records were obtained in each of the four experiments. This made a total of 80 lists used on each subject, or a total of 160 lists in all, involving the use of 6,400 different words. Experiments A, B. C and D were run in an irregular order so that the
the start, with which experimen
This irregular order was plann
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of experiments. The experiments were run during the winter of 1913-1914.

1. *Results Involving Recognition Memory*

In Table I are given the results from the four experiments segregated so as to show the effect of the four different intervals of time upon recognition. Each record for subjects *A* and *B* is based on five determinations, while the records for the average of the two are based, of course, on ten determinations. All these records are based on the correct recognitions that were made and no account is taken here of any incorrect recognitions. Experiments C and D were run primarily to study localization and do not lend themselves to an accurate estimation of the incorrect recognitions,—in Experiment D, of course, none being possible. It is fair to compare the four experiments in terms of these figures, although it must be understood that they are all too high to represent true recognition, as they are composed of two components, (1) true recognition and (2) pure chance. In Table II we have the records of Experiments A and B corrected for incorrect recognitions.[6] These data may be compared directly with the 1913 article. In Table III are given the latter data for 5 minutes, 1 day, 2 days, and 4 days, together with the gain made in the present investigation over the 1913 investigation.

a. Practise Effect in Recognition Memory. A study of Tables II and III and Curves Ac and 1913 in Plates I and II shows a most decided gain in recognition memory ability. In fact, the gain is very surprising. The introspections reported by the subjects do not indicate that they were aware of any particular progress in this regard. At the start (including the first 20 lists) subject *A* did not feel that she was doing so well as in the earlier work. After lists 21-24 had been marked she reported that she had "quite gotten back to her method of associations as in the old experiment." This set was interesting because of the sureness with which she recognized. She went through no list twice, but "just put down 1, 2 and 3 as they came along." To what extent a person could develop his ability to recognize is an interesting problem. Apparently it can be developed very much more and very much more easily than recall memory. It is also interesting to note that subject *A*, who was markedly superior to subject *B* in all these tests, improved at least twice as much as did subject *B*. The result we are finding in all our work in psychology today holds good here also;—" to him that hath it shall be given."

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[6] See the 1912 and 1913 articles for a discussion of the methods used in these determinations. (See note 10 for reference to the 1912 article.)

b. Various Methods of Studying Recognition and their Effects on the Process itself. A comparison of the results in Experiments A and B (shown in Tables I and II and in Plates I and II) indicates that a larger number of recognitions can be made when one list is being tested at a time than when four lists are tested at the same time. Methods which involve presenting several lists at one time to be tested later at various times, or which involve presenting lists at various times to be all tested later at the same time, will, then, not give so high a percentage of recognitions as when one list is presented and tested before the second is taken up. The difference in the two methods affected the poorer subject (*B*) more than it did the better subject (*A*), actually on the average 7.6% as against 2.4%.

Upon studying the data of Experiments B and C it is apparent that there is no real difference between the results. If anything, the results in Experiment C are higher than those in Experiment B. This is rather surprising when it is realized that in Experiment B one knows just how old the 20 correct words in the list he is marking are, whereas in Experiment C the 80 correct words vary in age from 5 minutes to 4 days. It would seem from these figures that recognition is not affected by any sort of mind-set in which one adjusts himself to a certain age of the recognitions. Meyer[7] has found that preparedness favors the process of recognition. But his preparedness was quite different from that considered here. In his case he found that a word was better recognized if it followed the 'old' syllable which was its immediate antecedent in the learning series, than if it did not follow such an antecedent word. Our data would also support this view. Such preparedness can be based on the fact that because the first word came first certain associations were aroused by the second word that would not normally arise and so in the test the presence of the first word would aid in the same association again coming to mind when the second word was again encountered. But no such specific aid is to be thought of when one prepares to meet words a day old, or an hour old, as distinguished from words but five minutes old.

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[7] H. W. Meyer. Bereitschaft und Wiedererkennen, *Zeitschr. f. Psychol.*, LXX, 1914, 161-221.

If these results were characteristic alone of subject *A* they might be explained on the ground of a difference in attitude toward the two experiments. Subject *A* reported that in Experiment C she considered the recognitions as being of subordinate importance to the localizations. Consequently she marked the recognitions as her first inclination directed, whereas in Experiment B the recognitions were considered the important thing and there she took few chances. But as both subjects *A* and *B* reacted in the same way toward Experiments B and C, it is doubtful if the above attitude explains the findings here.

It is very clear from the tables and plates that a higher percentage of recognitions can be made in Experiment D than in Experiment C. In the former the subject knew that he had seen before all of the 80 words in the list, whereas in the latter he knew that he had seen before only 80 of the 160 words in the list. When the data are analyzed it is found that this result is not due to a greater number of guesses or judgments with a faint idea of being correct being made, but is due mainly to a larger number of "absolutely certain" recognitions being made in Experiment D than in Experiment C. In the case of subject *A* 55% of the possible recognitions were absolutely certain ones in Experiment C and 62% in Experiment D; in the case of subject *B*, the corresponding figures were 32% and 45%. That knowledge that you couldn't make a mistake should affect the absolutely certain recognitions of trained subjects is an interesting fact. It may be, however, as suggested below, that if we could calculate the true recognitions aside from mistaken and chance ones that there would be little difference between the two experiments.

c. Conclusion. Recognition memory is susceptible of an enormous amount of improvement. This improvement is due, as was pointed out in the 1913 article, to improvement in reacting to the stimulus words and to improvement in methods of estimating the certainty of one's recognitions.

Recognition is more successful when one test is finished before starting the next. It apparently is more successful when one knows he can't make mistakes. And from our figures it appears that one can recognize words of varying ages mixed in together just as well as when they are segregated into groups all of the same age. That is, that a mind-set regarding the age of the words to be reacted to is not of advantage in recognition. If this is true, it would further distinguish the process of recognition memory from that of recall memory.

2. *Results Involving the Localization of Recognitions*

The localization discussed here refers specifically to the ability to tell whether a particular word had been seen 4 days, 2 days, 1 day, or 5 minutes ago in a list of 20 words, a list which had been read through but once. The 80 words that were judged were composed of 20 words from each of the four stimulus lists. In Experiment C they were mixed in with 80 new words, while in Experiment D they entirely constituted the reaction list. In the latter test there was no opportunity to make a mistake in recognizing the several words in the list of 80, but there were three chances in four of mistaking how long ago the word had been seen.

In Table IV is shown the per cent of words that were localized correctly in the two experiments for the four different intervals of time. In Table V the same information is given, but here based on corrections for all incorrect localizations. The first line in this table means then that 88.8% of the 20 words tested 5 minutes after reading them were really localized correctly as being in that list in Experiment C by Subject A. Likewise 91.3% were correctly localized by that subject in Experiment D. The minus per cent (—1.0) in the case of Subject B, Experiment D, for 2 days means that he made more incorrect localizations than correct localizations.

It is clear from these tables and Curves CL and DL in Plates I and II that ability to localize when an interval of 5 minutes is considered is nearly, if not quite, synonymous with ability to recognize when an interval of 5 minutes is considered. This fact is more clearly realized if we compare the ability to localize as found here with the ability to recognize as found in the 1913 article (Curves 1913), as well as with the results obtained in this article (Curves A and B). The ability to recognize shown in the latter curves represents far more training than the ability to localize shown in Curves CL and DL. But now when we compare recognition and localization for other intervals of time than 5 minutes we find that ability to localize fades out very much faster than does the ability to recognize. It is only natural to expect this. In recognition one needs to know only that he has seen the word before; in localization one needs to know not only that, but also just when one has seen the word.

One of the writers feels that the results shown here as to localization mean that the ability fades out just as does that of recognition, only faster, and that if we had data for intermediate intervals of time they would fit into our present data. The other writer believes that the words in a list with the shortest interval of time under discussion at any time will be localized very efficiently, that the words in the list with the next shortest interval of time will be localized with only a small degree of accuracy and from then on the localizations will be practically nil. In other words the latter believes that if the intervals had been 5 minutes, 1 hour, 2 hours, and 4 hours instead of 5 minutes, 1 day, 2 days, and 4 days, practically the same results would have been obtained for the first series of intervals as were found for the second series.[8] Which view is correct must be determined by future experimentation

From Table I it is clear that more correct recognitions are made in Experiment D than in Experiment C. In Table IV this same relationship is maintained as to localizations. But when corrections are made for the incorrect localizations which were made (shown in Table V) it would appear that there is no particular difference in the number of localizations that can be made in the two experiments. This fact that there were a greater number of mistaken localizations in Experiment D than in Experiment C suggests that if we had a way of estimating the mistaken and chance recognitions in both experiments that possibly there would be little or no difference in the results from the two experiments.

In Table VI is shown the per cent of correct localizations, correcting for incorrect ones, which can be made according as the recognitions, upon which they are based, are "absolutely certain," "reasonably certain," "faint," or "pure guesses." From these results it is clear that almost all true localizations are based on absolutely certain recognitions. It is also true that when the recognitions are incorrect the localizations that are made are very seldom accompanied by a higher degree of certainty than a "3"—"faint certainty." *Localization is then dependent on the same factors that constitute absolutely certain recognitions.*

As far as introspections go in this matter it would appear that the localizations were largely made in terms of events that happened on the day the words were read. We localized the word as having appeared at a certain time because the word and its associations had something to do with the events which had accompanied reading that list, i. e., a word called up a kitchen utensil and we remembered that we had been in the kitchen while reading the list, hence the word belonged to the list read on that day. However, the mental process was different from this analysis in that one did not ordinarily go through these steps in making his localization but seemed merely to be aware of the whole thing. The awareness, however, was very often reinforced by argument of this sort. In a few cases certain words were recognized as having been seen together in the same list and so if anyone was localized definitely, the others were then localized. But this manner of localization did not have the same sort of consciousness as the first type referred to and one felt as though he was making an intellectual judgment after weighing evidence rather than simply localizing.

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[8] This article has been withheld from publication for two years in the hope that this point might be cleared up before reporting the experiments, but as it seems impossible to find time to complete the necessary experiments, it is now published as it is.

III. General Discussion

1. *Concerning the Nature of Recognition.* Feingold[9] finds that " recognition ability varies inversely as the number of objects perceived," " inversely as the number of objects exposed," and " directly as the temporal length of perception." These conclusions are true in his experiments where only a few items are being studied and where the exposure-times are very short, but they are not strictly true when the conditions of experimentation are changed so that many items (20 to 150) are being studied or where the exposure-time is relatively long. As regards the first two statements it has been found[10] that as you increase the number of objects presented the number recognized is decreased, but the rate of decrease is much slower than the rate of increase in the number of objects presented. It is not an inverse proportion. As regards Feingold's last statement, we have found that this is true as one increases the exposure-time from one to two or three seconds per object shown,[11] but that it does not hold

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[9] G. A. Feingold. Recognition and Discrimination, *Psychol. Rev. Monog.*, 1915, No. 78.

[10] E. K. Strong, Jr. The Effect of Length of Series upon Recognition Memory, *Psychol. Rev.*, XIX, 1912.

[11] Exposure of 1 advertisement per 1.0 second resulted in 2.1% of the advertisements being recognized, whereas exposure of 1 advertisement per 3.5 second resulted in 6.3% of the advertisements being recognized. (E. K. Strong, Jr. The Effect of Size of Advertisements and Frequency of their Presentation, *Psychol. Rev.*, XXI, 1914.)

in anything like a direct proportion after that interval of time. In an experiment in which 25 advertisements were shown (1) at rate of one per second and (2) at the leisure of the subject—amounting to at least 5 seconds—the subjects being required thereupon to recognize cut-up pieces of the original advertisements mixed in with cut-up pieces of new advertisements, in the first case 17.3% of the pieces were recognized and in the second case but 34.2% of the pieces were recognized.[12] The relationship here between exposure-time and number recognized is far from a direct proportion. We would suggest that the conclusions of Feingold and ourselves probably show that until the subject has time to clearly perceive what is before him there is something like a direct proportion between efficiency in recognition and exposure time and an inverse proportion between efficiency in recognition and the number of objects exposed, but that as soon as the time exceeds this amount the proportion between efficiency in recognition and exposure-time, or inversely as the number of objects exposed, becomes less and less a direct proportion. Possibly one might say that Feingold's conclusions cover conditions involving maximum attention, whereas ours are based on conditions so extensive as to make impossible such a state of attention throughout the experiment.

Feingold also reports that the "recognitive ability varies inversely as the degree of similarity" between the first percept and the substituted one, that "the process of recognition is affective and is independent of cognition," and that "the feelings of familiarity and strangeness inhibit each other by fusion." According to his view these things are so since a stimulus-word arouses from one to many associations when it is seen the first time and it calls up again the first set of associations, only some of them, or none at all, according as the reaction word is similar to the first or not in mechanical make-up or meaning. If the stimulus-word and the reaction-word are identical then the nervous current traverses the same pathways a second time. Recognition results. If the reaction-word is different the nervous current traverses new pathways. Under the conditions of Feingold's experiments where eight words were shown each time, the feeling of strangeness is experienced. When the reaction-word is to some extent similar to the stimulus-word then we have a blending or fusion of the feelings of familiarity and strangeness according as part of the second discharge traverses part of the former pathways and part traverses new pathways.

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[12] E. K. Strong, Jr. An Interesting Sex Difference. *Ped. Sem.*, XXII, 1915, 521-528.

This fits in exactly with the speculations of the writers. In the 1913 article the process was likened to an electric current in that "if the current does not flow somewhere there is no current in the wire." And later it was suggested that "the recognition is more likely made while the accompanying associations are coming to consciousness the second time." All of this suggests Höffding's theory that recognition was due in some way to an increased "ease" of nerve functioning.[13] Having no evidence to support this notion of "ease" our speculations were not presented. Since then, during March, 1914, the following experiment was carried through which seems to throw some light on the subject.

Lists of 20 rather difficult words were made up. They were read one at a time by the experimenter to the subject who responded according to the free-association procedure. The reaction time was measured by a stop-watch. As few of the reaction times were less than one second, due to the unfamiliarity of the words used, the stop-watch gave us fairly reliable figures. Another list of 60 words made up from three of the 20-word lists was read later so as to come 5 minutes after one of the shorter lists, 1 hour after another, and 1 day after another. A comparison was then made as to the gain or loss in reaction time according as the response was *identical*[14] the second time with the first response or different from it. A and B served again as experimenter and subject on each other. At this time they were experienced subjects in the free-association experiment. The data indicate that there was a decrease in reaction time of 0.10 seconds when the same reaction word was given again to a particular stimulus one day later, this decrease was 0.18 seconds when an hour intervened, and 0.24 seconds when but five minutes intervened.

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[13] H. Höffding. Zur Theorie der Wiedererkennen. *Phil. Stud.*, VIII, 1893, 86-96.

[14] It has seemed best to consider in this experiment only those cases where the responses in both cases were identical. But in doing so we realize that recognition would have taken place in many of the other cases. We had in the experiment responses the second time (1) that were identical with the response the first time, (2) that were similar (in sound, meaning, etc.) to the first response, and (3) that were entirely different. Where to draw the line between the second and third group is a difficult matter. In many cases it cannot be done with certainty. For example, the first response to "kitchen" is "stove" and the second time it is "range." The subject reports that both "stove" and "range" were in mind both times but they appeared in reversed order. In such cases recognition would undoubtedly have occurred. But there are other combinations logically similar to this one for which the subject had no explanation. In order to be on the safe side, then, only those cases were considered where there was an identical response in both cases.

For Subject *A* the medians were, respectively, 0.11, 0.17, and 0.25 seconds, and they were for Subject *B*, respectively, 0.10, 0.20, and 0.23 seconds.

Now what has all this to do with recognition memory? If recognition is due to the arousal a second time of certain associational pathways and it is due to the increase in " ease " of the nervous discharge, then why may not this ease be a function of the speed of discharge, and if so, would not there be a corresponding increase in the rate of discharge with the per cent of recognitions that can be made? Now this is apparently what we find in this free-association experiment. In the 1913 article we find that

72.7%	of 20 words	are recognized after	5 minutes,	A ratio of	100 to
57.3%	"	"	1 hour	"	79 to
28.8%	"	"	1 day	"	40.

and in the same way, we find that there is a gain in reaction-time of

0.24 seconds	when the	association	is repeated after	5 minutes,	100 to
0.18	"	"	"	1 hour	75 to
0.10	"	"	"	1 day	42.

The relationship between recognition ability for these three intervals of time and the difference in association time for the two reactions for these intervals of time is identical.

Just what does all this mean? We have carried on the free-association experiment in such a way (only considering the cases where the responses were identical) that all the objective requirements for recognition have been fulfilled. Then we find that the differences in reaction-time between the two presentations with various intervals of time correspond exactly with the ability to recognize with those same intervals of time. Put in other words we have found that there is a direct relation between fading recognition and decrease in the gain of association time for a second trial. May we not say then that recognition is based on the " ease " with which the nervous current discharges the second time as compared with its discharge the first time. And if so, we must say that this element of ease is objectively measured largely, if not entirely, in terms of reaction-time. It is estimated subjectively in terms of the consciousness of recognition.[15]

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[15] It may be urged against these results that they are only a coincidence. Possibly this is true. We realize that this new evidence of

We can have the following situations involving recognition memory:—the first reaction to a stimulus, the second reaction to the same stimulus, the third reaction, . . . the nth reaction. We know that as we proceed from the 1st to the nth reaction the reaction-time will steadily drop (practise-effect). We know also that accompanying the first reaction there is often a consciousness of strangeness or novelty, accompanying the second reaction a consciousness of familiarity or recognition, and accompanying the nth reaction a consciousness of extreme familiarity, but different from that of recognition. As an example of this extreme consciousness of familiarity is the consciousness which accompanies meeting our father, our home, etc. Can we not say then (1) that when a stimulus causes a nervous discharge never experienced before we can be aware of the fact that the discharge occupies more time (or precedes with less "ease") than ordinarily and that this particular form of awareness is the consciousness of strangeness or novelty; (2) that when a stimulus causes a nervous discharge which has occurred only once, or only a few times, or a number of times a long time ago, that the discharge occupies less time than in the first case but still with more time than ordinarily and that this particular form of awareness is the consciousness of recognition; and (3) that when a stimulus causes a nervous discharge which has occurred many times it occupies about the same time that the reception of most of the things we perceive does and ordinarily there is no awareness of the "ease" element in the nervous discharge. When our attention is called, however, to this phase of the reception we experience the consciousness of familiarity, or "at homeness," characteristically different from mere recognition.

This explanation of recognition follows that of several writers. Höffding[16] first stated this view when he maintained

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the facilitation theory is indirect and circumstantial. Nevertheless we cannot help but believe that the experimental results do bear on the point. Our own introspections, as well as the introspections of those who know nothing of the nature of recognition, all point to a factor of ease in the recognition process. And "ease" necessarily implies less time. Other things being equal, one process cannot be easier than another without also involving the factor of being done quicker.

The term consciousness is used here as there is apparently no technical term to cover this particular case. Feingold employs the term "feeling." But there are many objections to this term, especially as psychologists have endeavored to restrict this term to the general consciousness of pleasantness-unpleasantness.

[16] H. Höffding, *op. cit.*

that the perception of an old object was qualitatively different from the perception of a new object. This qualitative difference (his Bekanntheitsqualität) was due, he claims, to a fusion of the perception with an immediately aroused image of itself. The Bekanntheitsqualität is aroused because in this fusion there is an increased ease of nerve functioning. Dearborn[17] and Meumann[18] have also distinguished between the consciousness of familiarity and strangeness on the basis of an easiness with which the mental process proceeds.

TABLE I

Showing the percentage of twenty words that can be recognized after four different intervals of time. (No correction is made in these figures for the incorrect recognitions which were made.)

Subject *A*

Interval of time	Experiment A		Experiment B		Experiment C		Experiment D	
	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.
5 minutes......	96.75	1.13	97.00	0.80	95.28	1.10	94.26	1.13
1 day..........	65.75	1.20	60.00	4.17	65.52	4.70	74.77	3.57
2 days.........	55.00	2.50	47.25	5.23	48.78	1.87	53.28	1.09
4 days.........	41.50	3.43	45.00	3.67	41.02	2.86	49.26	4.97

Subject *B*

Interval of time	Experiment A		Experiment B		Experiment C		Experiment D	
	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.
5 minutes......	79.00	3.57	80.00	1.83	83.95	1.97	86.5	2.20
1 day..........	41.25	3.17	35.50	2.27	39.25	1.80	55.75	4.13
2 days.........	38.50	2.73	21.50	1.73	28.00	3.70	50.5	3.53
4 days.........	33.75	1.67	25.25	1.57	20.25	2.27	40.5	3.80

Average of Subjects *A* and *B*

Interval of time	Experiment A		Experiment B		Experiment C		Experiment D	
	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.
5 minutes	87.88	2.33	88.50	2.13	89.62	1.42	90.38	1.30
1 day..........	53.50	3.05	47.75	3.64	53.39	3.67	65.26	3.19
2 days.........	46.75	2.63	34.38	3.94	38.39	3.00	51.89	1.88
4 days.........	37.63	1.79	35.13	2.86	30.64	2.66	44.88	3.29

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[17] G. V. Dearborn. Notes on the Discernment of Likeness and Unlikeness. *Jour. Philos. Psychol. and Sci. Methods*, VII, 1910, 57-64.

[18] E. Meumann. Ueber Bekanntheits und Unbekanntheitsqualität. *Arch. f. d. ges. Psychol.*, XX, 1911, 36-44.

TABLE II

Showing the percentage of twenty words that can be recognized after four different intervals of time, *when a correction is made for incorrect recognitions.*

Interval of time	Subject *A*				Subject *B*				Average Subjects *A* and *B*			
	Expt. A		Expt. B		Expt. A		Expt. B		Expt. A		Expt. B	
	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.
5 minutes	96.00	1.43	96.50	0.93	76.31	4.24	78.70	1.75	86.16	2.71	87.60	2.20
1 day	59.89	1.48	52.97	4.22	33.04	2.88	30.67	2.23	46.47	3.58	41.82	3.44
2 days	40.35	4.05	38.01	7.12	27.33	2.63	16.23	2.00	33.84	2.69	27.12	4.29
4 days	33.89	2.80	34.37	3.03	27.54	1.51	16.45	1.57	30.72	1.80	25.41	3.05

TABLE III

Showing (1) the percentage of twenty words that can be recognized after four different intervals of time from the 1913 *Psychological Review* article and (2) the gain made in the present investigation over the earlier study, i.e., the difference between the data in Experiment A (Table II) and the 1913 data.

Interval of time	Subject *A*			Subject *B*			Average Subjects *A* and *B*		
	Expt. A (1913)		Gain	Expt. A (1913)		Gain	Expt. A (1913)		Gain
	Per cent	P.E.	Per cent	Per cent	P.E.	Per cent	Per cent	P.E.	Per cent
5 minutes	73.9	5.2	22.10	66.3	5.4	10.01	70.1	3.7	16.06
1 day	28.9	4.6	30.99	21.4	5.6	11.64	25.2	3.7	21.27
2 days	22.3	3.5	18.05	21.1	2.6	6.21	21.7	2.0	12.14
4 days	22.9	3.4	10.99	16.0	2.0	11.54	19.5	2.1	11.22

TABLE IV

Showing the percentage of twenty words that can be localized as to when they were previously seen for four different intervals of time. (No correction is made in these figures for the incorrect localizations which were made.)

Interval of time	Subject *A*				Subject *B*				Average Subjects *A* and *B*			
	Expt. C		Expt. D		Expt. C		Expt. D		Expt. C		Expt. D	
	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.
5 minutes	89.25	2.20	91.75	1.63	78.5	1.37	75.0	3.67	83.88	1.59	83.38	2.56
1 day	19.5	2.43	24.25	2.13	12.25	0.60	16.25	2.67	15.88	1.37	20.25	1.81
2 days	3.0	0.27	4.75	1.23	4.75	0.73	7.75	0.60	3.88	0.42	6.25	0.69
4 days	2.25	0.60	3.25	1.23	4.25	0.69	6.75	1.13	3.25	0.50	5.00	1.06

TABLE V

Showing the percentage of twenty words that can be localized as to when they were previously seen for four different intervals of time, *when a correction is made for incorrect localizations.*

Interval of time	Subject *A*				Subject *B*				Average Subjects *A* and *B*			
	Expt. C		Expt. D		Expt. C		Expt. D		Expt. C		Expt. D	
	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.	Per cent	P.E.
5 minutes . .	88.81	2.32	91.25	1.50	77.71	1.70	73.9	4.05	83.26	1.61	82.58	2.61
1 day.	17.68	2.53	19.38	1.71	10.07	0.63	10.18	2.28	13.88	1.38	14.78	1.61
2 days.	1.92	0.25	2.7	1.04	2.63	0.63	—0.97	0.77	2.28	0.34	0.87	0.61
4 days.	1.84	0.61	2.85	1.10	2.08	0.43	3.78	1.23	1.96	0.42	2.41	0.86

TABLE VI

Showing to what extent localization can be correctly made as depending on the degree of confidence of the recognition.

Certainty of recognition	Subject *A*		Subject *B*		Average Subjects *A* and *B*	
	Localization correct	Localization incorrect	Localization correct	Localization incorrect	Localization correct	Localization incorrect
Absolutely certain. . .	25.8%	0.6%	22.2%	0%	24.0%	0.3%
Reasonably certain. .	2.5	0	0.9	0.2	1.7	0.1
"Take a chance". . . .	0	0	0.2	0.2	0.1	0.1
Pure guess.	0	0	0	0	0	0

Woodworth[19] has given a somewhat similar explanation of recognition. He says, "When a fact arouses the same associations, or reactions, as it did on a previous occasion, it is recognized. It is the reaction that gives the feeling of familiarity. According to this view, it is not the associations leading to the fact, but those leading from the fact to some reaction, that lie at the basis of recognition. It is what the fact does within us that determines whether it shall be recognized or not, if it does something that it has done before, i. e., leads to the same reaction as before—it is recognized; but if it does something new—leads to a new association or new perception—it is not recognized. The recognition depends on the transition from the fact to some reaction; if this transition is one which has been frequently or recently made, it is now made with a sense of having been made before, and this is the basis of the feeling of familiarity and of the recognition." . . . "In neural terms, it is the passage of a current along a long unused brain path or synapse that gives the feeling. The passage of a synapse *feels* differently, according as it is constantly in use, has never been used, has only recently been used, or has been much used at some earlier time but has since fallen into disuse."

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[19] R. S. Woodworth, *op. cit.*, p. 72-73.

It seems to us that most writers who have espoused the facilitation theory have laid themselves open to the criticism that the facilitation theory does not help us on the side of consciousness by their not making an attempt to correlate "ease" with "consciousness."[20]

Woodworth answers this objection by postulating that the passage of a current across a synapse *feels* differently according to the degree with which it has been used. We state the same thing a little differently by stating that the rate of discharge of the current across the synapse has a conscious correlate in the varying qualities of consciousness, i. e., of strangeness and familiarity. With us there is no greater difficulty in saying that when certain cells or synapses are aroused quickly or slowly that the consciousness of recognition enters into consciousness than in maintaining that when certain cells or synapses are aroused certain ideas enter consciousness.

We should distinguish cognition from feeling, to use these old terms, by saying that when the object is presented a second time, the cognition is the result of certain pathways that are aroused,—that it covers the total reaction of the organism to the stimulus; but that the feeling is the result of the ease with which the reaction is made. If every time an electric current passed over a wire it reduced the resistance then we could express the above by this analogy. When a sending key is struck the answering click of the receiving key would correspond to the idea or ideas aroused, would be the reaction; but the heating of the wire due to its resistance to the current would correspond to the feeling. As the heating becomes less and less with the lessening resistance, so the

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[20] See in this connection, E. L. Woods, An Experimental Analysis of the Process of Recognizing. *Amer. Jour. Psychol.*, XXVI, 1915, 313-387. Miss Woods presents two objections to the facilitation theory. First, "It helps us not at all on the side of consciousness," and second, "the most facile processes,—the processes which accompany those daily, habitual experiences to which our reactions are all but automatized,—do not bring with them the peculiar experience of familiarity." We believe we have answered the first objection in the paragraph accompanying this note. We would emphatically deny the truth of the second objection. We believe there is a characteristic feeling of 'at-homeness' which accompanies perception of very familiar objects. We would admit, however, that because of the fact that attention is seldom directed to habitual performances this feeling is seldom specifically noted. But it is possible to so note it, if one chooses. Possibly homesickness is nothing but the loss of this affective element so common in our lives and so little attended to ordinarily, until it is absent.

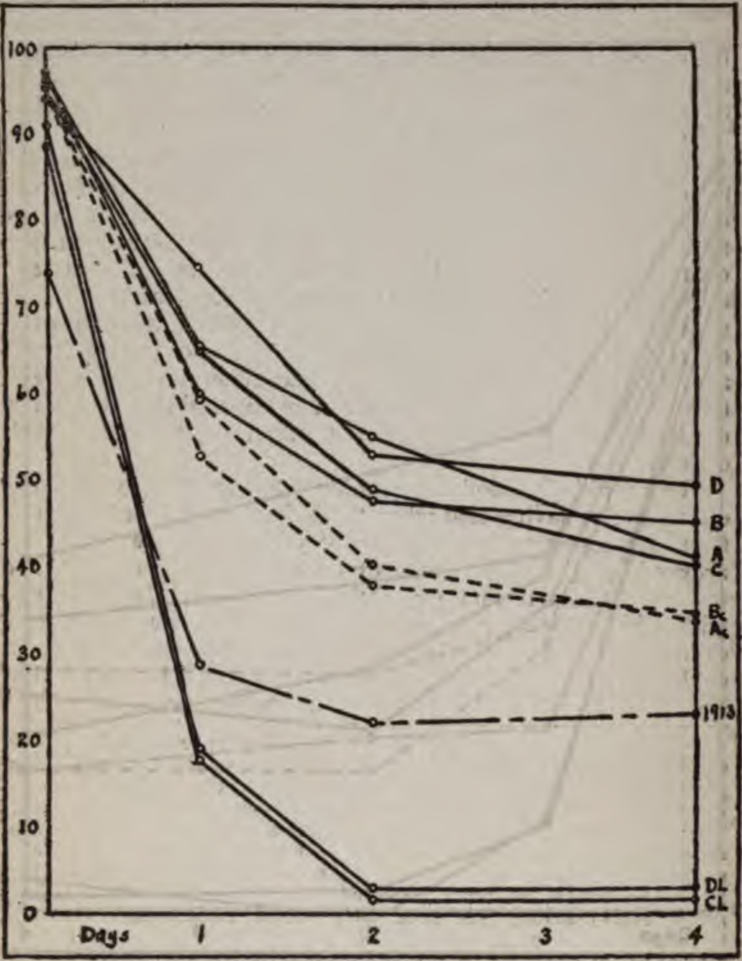

PLATE I.—Subject *A*. Curves A, B, C, D, present recognition memory data (uncorrected for chance errors) from Experiments A, B, C, and D, respectively. Curves Ac and Bc present corrected data from Experiments A and B. Curve 1913 presents corrected data from 1913 article. Curves CL and DL present data on localization from Experiments C and D, respectively.

feeling would correspondingly change. And as different degrees of heat give off different colors, so different degrees of feeling could produce different types of awareness in consciousness, i. e., strangeness, recognition, "at-homeness."

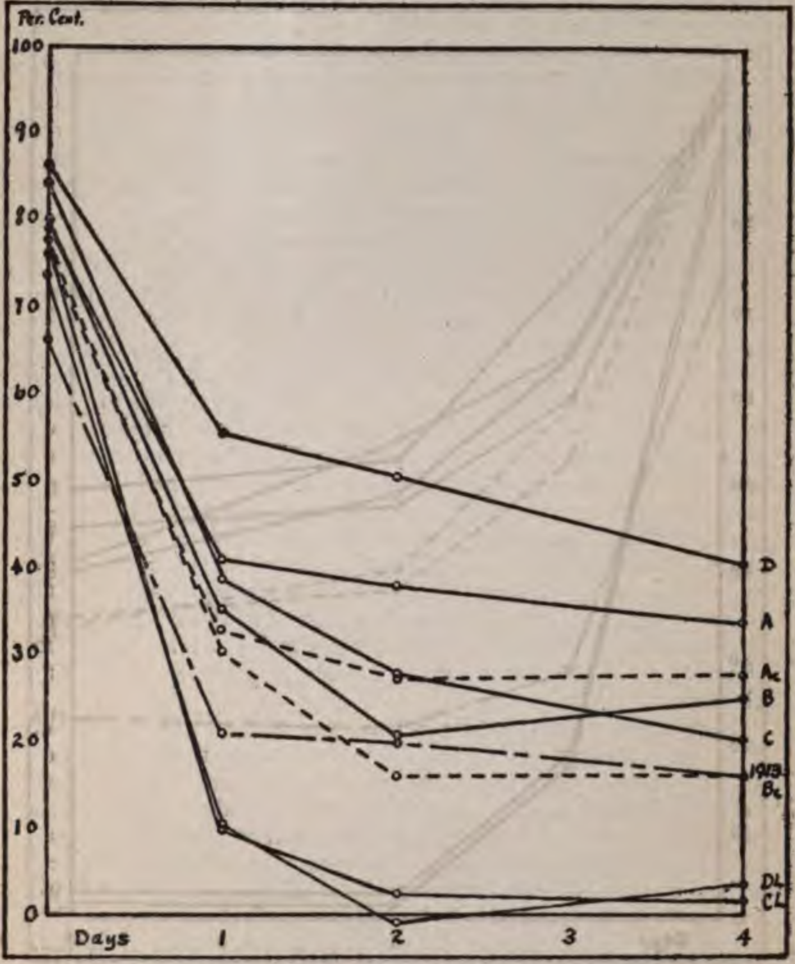

Plate II. Subject *B*. Curves A, B, C, D present recognition memory data (uncorrected for chance errors) from Experiments A, B, C, D, respectively. Curves Ac and Bc present corrected data from Experiments A and B. Curve 1913 presents corrected data from 1913 article. Curves CL and DL present data on localization from Experiments C and D, respectively.

The whole controversy as to whether images must be present or not is answered by simply affirming that whether the discharge over the associational pathways results in the arousal of images or "imageless" thoughts is immaterial to the explanation of recognition. In fact we should be willing to add on the basis of our own introspections and those of others that this discharge might not be strong enough to bring any image or imageless thought into consciousness, but yet be sufficient to arouse the consciousness of recognition. It is not the reaction in thought or action that is important, it is the difference in ease of the nervous flow between this particular discharge and other discharges which determines whether familiarity or strangeness is present.

In running over such a classification of the theories of recognition as Miss Woods[21] gives, it seems to us that the position maintained here includes most of the theories listed by her. Much of the controversy in the past over this subject has been between those who maintain that recognition is due to ease and those who maintain that recognition is due to some sort of fusion between the new percept and the old, or to the arousal of associations previously present. As we have already pointed out, we believe all these factors enter into the process of recognition. The object must be perceived as the same object it was previously perceived to be—there is then fusion of percept and image, if you wish to speak that way; there is the arousal again of the same associations previously aroused; and there is an increased facility of nerve functioning. All are necessary. Without the former the latter would not occur, but recognition itself is due to the latter,—is the conscious correlate of the ease-difficulty element.

Conclusion. Recognition is then due first of all to the fact that when an object is seen again it arouses the same associations that it did when it was first seen. The nervous current traverses the same pathways again that it did before. This condition is essential for recognition but it is not the explanation of the recognition itself. The recognition arises as a conscious awareness that the second discharge has flowed more easily than it would if it had been a discharge to a new object and it has flowed with more difficulty than if it had been a discharge to an old familiar object. This ease may be measured objectively in reaction-time. It is estimated subjectively in terms of consciousness of strangeness or familiarity.

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[21] E. L. Woods, *op. cit.*

2. *Concerning the Nature of Localization.* From our data it appears that one can localize words in the 5 minute interval list practically as accurately as he can recognize the same words, but that this ability to localize fades out very much more rapidly with the lapse of time than does the correspondingly ability to recognize.

The two writers are disagreed, however, as to whether the loss in localization ability is due actually to the lapse of time, as in recognition or recall memory, or whether it is due to the fact that localizations cannot be distinguished between lists of varying ages, except in the one case when the last list is concerned. The second writer believes that to the extent that one can localize, which according to our data is practically nil after a two day interval, it can be done in some way or other by estimating the strength of some mental process and so assigning the length of time since the object was seen before. The first writer believes that one can only estimate the strength of such a mental process as being in a given case the strongest of all or the next strongest, and that beyond that one cannot go.

Both writers are agreed, on the other hand, that localization is akin to recognition in being primarily such a state of consciousness as is included under the head—affective. It may involve more than this. It is recognized also, as pointed out above in the introspections, that localization may be made at times on the basis of associations which link the particular object to other objects whose time relations are known. But such localizations with us were introspectively different from those based on mere awareness that the word had been seen in such and such a list. We should say, then, that a person is capable of being conscious (1) that he has seen an object before and (2) that he has seen this object not recently, but some time ago, or vice versa. Possibly this second ability is based on some sort of an estimation of the strength of the process which produces the consciousness of recognition.

From all of our experiments and from those of Feingold it is clear that one seldom identifies a new object as having been seen previously. The consciousness of strangeness is clear and unmistakable. In terms of our theory the ease-difficulty element involved in a first discharge is quite different from that element as involved in a second or third discharge. And as we know from studying learning curves that the greatest drop in reaction-time is between the first and second trial and that with each successive trial the drop becomes less and less, so here we may liken the consciousness between strangeness and familiarity as due to a large difference in the ease-difficulty element, whereas when it comes to estimating the amount of familiarity, and possibly this is what localization as a feeling is, we have an increasingly difficult task due to the smaller and smaller actual changes in the ease-difficulty element with each successive reaction to the given object (or in terms of the forgetting curve, we would have correspondingly an increasingly difficult task due to the smaller and smaller actual changes in the ease-difficulty element with each succeeding unit of time). If this hypothesis is true (and we are not at all sure that it is), localization is not a qualitatively different type of consciousness from recognition, but is a finer subjective analysis of the consciousness of recognition. That is, when we recognize we are roughly aware that the associational discharge has flowed easier than it would in response to a new object. When we localize we estimate very carefully the amount of this "ease" element and in terms of it say we have seen this object recently or a long time ago.

PSYCHOANALYTIC STUDIES OF GENIUS.

By Lucile Dooley, Clark University

The material gathered under this title and published herewith is a collection of epitomes, or abstracts, of essays on the psychology of great men, which have appeared from time to time during the last decade, for the most part in German psychoanalytic periodicals. The purpose in gathering them thus and presenting them in brief has been two-fold; first, to introduce this important branch of psychoanalytical literature to those who do not readily read, or have access to, the German; and, second, to use them as an introduction to a study of genius from the psychoanalytic viewpoint, which the writer is preparing, of which the principal part is an analysis of the character and genius of Charlotte Brontë.

I have not exhausted, in this collection of epitomes, the psychoanalytic studies that refer to well-known men and women. But those remaining are, for the most part, more concerned with pathology than with the character and genius of the individual, or else they explain the motivation of some particular production of the subject, without giving any analysis of the character of the author. Two of those I have given here, indeed, came under this latter category. They are "Jensen's 'Gradiva'" by Sigmund Freud and "Hamlet and the 'Oedipus-Complex'" by Ernest Jones. But these two essays are of fundamental importance as having initiated and stimulated this movement in the study of genius.

The first indication of the possibilities for psychoanalysis in art, literature, and biography, in fact, was found in Freud's "Interpretation of Dreams," which remains as the embryo out of which the whole structure of non-pathological applications of psychoanalysis has grown. Freud gave a definite impulse to this particular new line of psychological analysis when he brought out his beautiful analysis of "Gradiva," following it with his "Leonardo." Ernest Jones took up the hint dropped in the "Interpretation of Dreams" about Hamlet, and elaborated his own admirable study. It has proved a most inviting field, and one in which there still remains much unbroken ground.

I

THE DELUSION AND THE DREAMS IN JENSEN'S "GRADIVA"

By S. Freud, 1907, *Schriften zur angewandten Seelenkunde*, I, 81 p.

Among those "dreams that never were dreamt" but were attributed by an author to the characters in his tale, few lend themselves more perfectly to the uses of the psycho-analyst than those in the "Gradiva" of Wilhelm Jensen, a little story which he calls a "Pompeian Fantasy." Not only the dreams but the plot of the story might come from an idealized clinical record. The story runs as follows:

A young archeologist, Norbert Hanold, who cares only for his studies, who shuns all social life and seems unaware that women exist in the world of reality, finds an ancient bit of sculpture, representing a young girl in the posture of walking, which takes a strong hold upon his imagination. "He finds something modern in it," as if the artist had copied it on the street, "from the life." (p. 2.) He calls the maiden "Gradiva," "the one stepping forward," and fancies her the daughter of a patrician aedile of Pompeii, one who took his part in the service of the temple of Ceres (p. 2). He decided that she was undoubtedly Grecian. Gradually he became absorbed in speculation as to the origin of the piece and especially busied with the problem as to whether the artist had rendered it "after the life." To determine this point, so important, as he fancied, to his archeological research, he observed the gait and the feet of women whenever he could, and his failure to find a foot or a graceful gait like the Gradiva's filled him with distress.

Then he had a dream of Pompeii, in which he saw Gradiva "in her native town where she lived at the same time as himself." (p. 3.) He saw her climb the steps of the temple of Jupiter, lie down there, and become white as marble, and then he saw her buried by the rain of ashes. (p. 7.) When he woke he heard the song of a canary in a cage in a window opposite, and, glancing out, he thought he saw the likeness of Gradiva in the street. He hastened down but could not find her. The dissatisfaction thus aroused decided him to escape *his* cage and go upon a springtime journey to Italy—in the interests of science. (p 4.) On his journey he was much annoyed by his constant meetings with young couples on their honeymoon. Disgusting and inexplicable as their actions were, however, they aroused in him a feeling of greater dissatisfaction, of "something lacking," which impelled him restlessly ever southward till he came to Pompeii, realizing only then that this had been the goal of his journey all along, and that his purpose was to seek some trace of Gradiva—her foot print in the ashes, perhaps. (p. 10.)

And there, at midday, he saw her as he had seen her in his dream, which he had meanwhile forgotten. For some time he more than half believes her to be a phantom, which lives again only at midday. He offers her asphodel, the "flower of forgetfulness" and awaits her coming daily. Then he sees again one of the young couples, and the rose worn by the lady reminds him of Gradiva's words about roses when he gave her asphodel. He dreams that night that "somewhere in the sun" sat Gradiva, and she made a noose of grass with which to catch a lizard and said: "Please, be quite still—my lady colleague is right, this means is really good, and she has used it with the best results." (p. 18.) He had met an old scientist catching lizards that day. He learns, soon after this, by a gradual revelation from the girl, that she is *Zoë Bertgang,* daughter of a zoologist of his own university town. She had been his childhood's playmate and they had shared their meals together, cuffed, and teased, and loved each other. It is when she speaks of the canary bird in her window that the full memory dawns upon him, to which she has been carefully leading up, like one who cautiously awakens a sleeper from his dream. The story ends, as novels do, in a wedding.

In this story the building up of the fantasy about the little image is traceable to just the causes that a psychiatrist finds for the obsessions of his patients. The memory of the erotic experience of childhood was repressed into the unconscious by the conflicting tendencies developed at puberty. But, repressed, it was not quiescent, it constantly worked for a fulfillment of the childhood wish for the little playmate with whom his relations had so deliciously alternated between the aggressive and the tender. (p. 35 and 70.) The basrelief, by an unexplained accident, reproduces the characteristic gait of Zoë, which had unconsciously become to him a fetich. (p. 38.) His resistance is so strong that it creates almost a negative hallucination with regard to the actual object of his childish love, for he knows nothing of her, though she lives just across the street. The passion for study has been the conscious expression of his libido, but the direction of his study has been determined by the unconscious object, i. e., the study of ancient, buried things, comparable to the buried epoch of childhood. The placing of the relief in Pompeii is an expression of the wish to "dig up this buried past." (p. 42.) The delusion that Gradiva has been buried and comes to life expresses his wish, or hope, that she actually live. Herein is the explanation of the "modern" impression conveyed by the figure, while the subconscious memory leads him to dream of their having lived together in Pompeii, the native town of both. The representation of the father as devoted to the service of Ceres has symbolic connection with his profession of zöology. The name "Gradiva" is a translation of Bertgang. (p. 30.) (Bert=*glänzende*, gang=*schreiten*, the "glorious gait.") Zoë is a Greek name for *life*. The second dream is more complicated than the first one, but it too shows clear wish-fulfillment. Zoë is identified with her father, the lizard catcher. He himself is the lizard and he would gladly be caught. (p. 62.) The reference to the colleague (the words almost an exact repetition of those that Prof. Bertgang had employed the day before—p. 81) relates to the young bride. "Somewhere in the Sun" signifies the hotel "Albergo di Sole" which he has just discovered after noting, subconsciously, that "Gradiva" was not among the guests of any of the others.

The unconsciously motived actions of the hero are as significant as the dreams and the fantasy. His journey is a flight from the living love, which he is not prepared to accept, in search of her fantastic substitute. (54-57.) He offers her asphodel, symbolic of forgetfulness. He takes the young bride and groom for sister and brother when they no longer offend him—a new defensive mechanism. He seeks to know what guests are in all the hotels while he still believes Gradiva to be a phantom.

To complete the clinical parallel, Zoë follows the psychoanalytic method of catharsis for his restoration to sane and normal life. She gradually brings the buried memory to consciousness, "digging up something interesting," as she told her friend. (p. 21.) She transfers the deep emotion that weights the repressed memory and its delusional expression to herself, as a physician must do, but, unlike the physician, she does not have to bring about a further transference, and so her cure is completely satisfactory. The buried wish is not only brought to light, but it finds complete and literal fulfillment.

That the poet knew nothing of the psychoanalytic theory and method does not invalidate the conclusions of the study, but rather enhances their value. The story was a product of his fancy, as he says, that is, it is drawn from his own unconscious, and it has a completeness that is not always possible in a study from real life, where some facts are beyond our reach. The poet divines where the physician dissects, and both are interpreters of life. One great argument for the dynamic force of the unconscious is the fact that poets write so much better and larger than they know.

Jul 12 1917

II

A CHILDHOOD MEMORY OF LEONARDO DA VINCI

By SIGMUND FREUD, 1910, *Schriften VII,* 71 p.

To men of his own time, as to those of ours, the character of Leonardo da Vinci, one of the greatest artists and greatest men of the Renaissance, has been an insoluble enigma. Why did he find such difficulty in finishing his later pictures, leaving most of them unfinished? Why did he paint so laboriously and painfully, he, the great master? Why did he turn more and more away from painting and toward science, to the detriment of his art? What is the secret of the *Monna Lisa,* with her enigmatical smile? What did that smile mean to the artist, that he should portray it in almost every subsequent picture? Why did he, with his physical beauty and vigor, his personal daintiness, his genial nature, shun intercourse with women, and withhold his intimacy even from male friends? In a sensuous and exuberant age his cool deliberation and abstinence seem unaccountable. Geniality and coldness, gentleness and cruelty, strangely oppose each other in him. Glimpses of his private life, as revealed by his diary, show further contradictory traits.

These manifold problems the psycho-analyst undertakes to solve by his method of going back to the child life of the subject. At first glance the data at hand seem meagre enough, but there are significant aspects. Leonardo was an illegitimate child whose father was of much higher rank socially than his mother. The first four years of his life were passed with his mother alone. Then his father married a lady of his rank, and the marriage being childless, Leonardo was taken into the home, and was tenderly cared for. That is all that is certainly known and our clue would be most unsatisfactory were it not for a childhood memory that the artist has set down in some of his writings concerning birds and aeronautics. To quote his own words:

"It seems that I am predestined to be so thoroughly interested in the *vultures,* for as a memory of very early life it comes to my mind that while yet I lay in the cradle a vulture came down to me, opened my mouth with her tail, and many times pressed against my lips with this tail of hers." (p. 19.)

We are convinced that this strange story is more of a fairy tale than an actual reminiscence. It is not a memory of an actual event, but the memory of a fantasy formed after childhood and referred back to childhood because it expresses a wish of infantile sexuality, preserved because of its great emotional significance. We give it this interpretation because very similar formations have often occurred in the dreams of homosexual patients, and the psychoanalytic physician has learned to know the origin of the fancy about sucking the tail of a bird. The symbolic rôle of the vulture is explained by the mythology of several lands, but especially that of Egypt, wherein the goddess of Motherhood, *Mut,* was given the head of a vulture. The vulture was supposed by the ancients to be always a female and to conceive without union with a male. The passive rôle played by the child in Leonardo's reminiscence is significant. While other features of his life played some part in forming his character the relations with father and mother, sketched above, illuminated by this peculiar creation of his fancy, are seen to be the nucleus from which his most striking traits developed. An only, much loved and petted, child of his mother, he lacked in his infancy the masculine influence of a father. The overtenderness of the mother produced a precocious emotional, that is, sexual, development, with complete fixation upon the mother. The earliest expression of infantile sexuality, that of suckling the mother, remains untransformed in him, giving birth to this fantasy, which also represents another and more direct infantile idea of sexual relation with the mother. The homosexuality that often results from such mother fixations is seen in his passivity. He could not take the normal road of expression for sexual desire because he never outgrew the infantile passivity and the mother-ideal. Later he lived with his father and was for long the only child in the family; therefore he was loved and cherished by the new mother also. The kind stepmother became somewhat identified with his own mother and served to strengthen the mother-fixation. With puberty and the father's influence came partial freedom, and a sublimation into creative work. Yet he always remained unaccountably inactive in some respects (so his biographers say). He spent his affection upon the beautiful youths he gathered about him as pupils, playing a motherly rôle to them. During the early period of his life as a successful artist, including the time that he spent at the court of Ludovico Sforza, at Milan, the mother-complex was not dominant and sublimation was apparently successful.

But when he began the painting of the *Monna Lisa* something occurred that woke the dormant force and made it active, though never fully conscious. Upon this lady's face he saw the very smile of his mother, which was woven into the fabric of his dreams. (p. 47.) The long struggle to complete the picture was the result of an inward struggle in which the infantile love tried to reassert itself and the face of the mother strove to be put upon the canvas. This struggle affected every subsequent picture. The mysterious smile appears upon two faces in the *St. Anne Trio,* and this picture expresses clearly the conflict. (p. 49.) Here are *two* mothers claiming the little son, and the elder does not yield to the younger, for all her tenderness. So he, himself, had had two mothers, and so the conflict between the mother-claim and the claim of adult life is symbolized.

From this time he turns more and more to scientific research, making the requirements of his painting an inadequate excuse for his investigations. Such zeal for knowledge, psychoanalysis has taught us, is a form of *curiosity,* and when carried beyond utilitarian needs it is traceable in its beginning to the infantile sexual curiosity arising from premature development. (p. 65.) The great reinforcement to this natural tendency to scientific investigation which leads him to pursue it in the face of the authorities of his time is due to the rebellion against authority based upon resentment against his father—the representative of authority to the child. The attention he devoted to problems of *flying* is traceable also to the infantile conflict concerning the father. The vulture fantasy is one side of this, the other is the symbolization of masculine potency by flying. It was after the transference to the step-mother that this conflict arose. This change from creative work to study shows the regression in character, regression in expression of the libido, as the infantile mother-complex demar
expression and comes in conflict with later ideals. It is a retur
passivity. It was not only the experience with Monna Lisa
brought about this change. The removal of the father's infl
and then of the paternal influence of Duke Ludovico Sforza,

had tended to bring out his masculine character, played an important part. The conflict between these influences and the older mother fixation accounts for the contradictory traits of character. A minor point is the fact that his subconscious resentment toward his father "the Gentleman" led him to "out-herod Herod" by emphasizing all the outward and inward marks of a gentleman, fine apparel, gentle manners, refined habits, and well-ordered life. (p. 54.)

III

Giovanni Segantini

By Karl Abraham, 1911, *Schriften XI.*, 65 p.

"Among the artists of our time," says Dr. Abraham, "Giovanni Segantini looms up as a mighty, independent, personality. His development, his outer and inner life, his art, his work, are of such outstanding peculiarity that he presents to Individual Psychology a whole cluster of unsolved problems. To turn upon these the light of psychoanalytic principles is the goal of the present study. . . . An artist of genius, a great man—as Segantini was—has sufficient claim upon our interest in the fact that he is our contemporary. We shall make no mistake, however, if we expect to gain a rich addition to our general knowledge of the psychology of artists for our pains."

Segantini's aim was not to paint nature merely, although he was a great nature painter. He strove to paint the soul of nature, the meaning of it, or, as he said himself, "to conquer and declare *Work, Love, Motherhood,* and *Death.*" In these four words he names the springs out of which his artistic fancy was always fed and renewed. (4.) His life was ruled by the same powers as his art. For his extraordinary ability we cannot find the ordinary explanations. Parentage, education, advantages, give us no clue. He lost his parents when he was five years of age, was brought up none too carefully by his stepsisters, was placed for several years in a Reformatory as an incorrigible, had no real education. His youth was a constant battle with hostile powers. Segantini writes concerning this problem of his life, "They ask me how, in my almost wild life, I have developed Thought and Art? I do not know how to answer; perhaps one must dig down, for such an explanation, to the roots, through all experiences of the soul into the first, the most remote, activities of childhood." Following this clue we turn to his childhood.

As was said he lost his parents in his sixth year. Ever after he idolized the memory of the dead mother. He remembered her as a tall, delicate, sweet woman "lovely as a sunset in Spring." This last phrase becomes significant when we recall the frequency of sunset light in the pictures of his earlier and his last period, as we shall see. Motherhood and Death became associated in his mind. It seems that she was never strong after his birth, though she lived on for five years, so he always felt that he had been the cause of her death, and this conviction becomes greatly over-conditioned. Artist and neurotic have in their constitutions a strong tendency to this overweighting of an idea. In both the instinctive life is of abnormal strength, originally, and is, on the other hand, both repressed and sublimated to an unusual degree. Both artist and neurotic stand with one foot outside Reality, in a world of Fancy. (19.) In the neurotic suppressed fancies work out as disease symptoms; in the artist they find expression in creative work, but not in this alone, for the artist shows, almost always, some neurotic features. Thus Segantini, as artist and neurotic, has over-conditioned his ambivalent feelings toward his mother. He idolizes her beauty, her frailty, her sadness, but his self-reproaches for her death are over-compensations for death-wishes that he has had, in the forgotten years of childhood, against the mother who gave him little cherishing. (His memories of her make no mention of love and care.) Hereafter he paints delicate mothers and sturdy children as expressions of his self-reproach. He associates flowers with young mothers and once strikingly imaged this association in a strange vision, or illusion, when in the Alps. He saw a flower form upon the clouds and then before his eyes it was transformed into a Madonna. He painted this, putting an apple into the hand of the Child, and called the picture "The Fruit of Love." Nature and Motherhood and Death were twined together in his thought, and the three were one.

Turning to another episode of his childhood that is pregnant with meaning, we find him, at the age of twelve, making a drawing of a pretty dead infant, to gratify the weeping young mother. He felt no fear of the corpse, which is certainly unusual. and may perhaps betoken the development of the sadistic side of his nature. He drew the child as *living* and joyous, and felt, for the first time, that through Art the dead could be made to live. This experience strengthened the association between Motherhood and Death. His first picture drawn for the Academy was a Head of Niobe, *another* weeping mother! A tone of melancholy pervades his early pictures, pictures of shepherds lonely with their flocks, against sunset skies. Segantini has said of himself, "My spirit was nourished by a great melancholy, which resounded in my soul in unending sweetness." Tenderness, sweetness, sadness, and death, the motives embodied in his mother, thus ruled him. After his mother's death his father had taken him to the narrow dark streets of a great city, so he lost at once Mother, Home, and Nature. The homesickness for these he never outgrew and sought always to regain them in fancy.

But the second period of his artistic life was a period of joyousness, of color and light, and before we enter into this we must consider for a moment one or two experiences of childhood and youth connected nearly or remotely with his father. The father, after keeping him unhappily in the city for about a year, left him in the care of his stepsister, went to America, and was heard from no more. Segantini never mentions his father but we may guess what resentment may have filled his childish soul. The father deprived him of all that made life pleasant and then deserted him. He grew up an unruly, aggressive spirit, dreaming dreams of future greatness.

He was always a radical, always independent, going his own way, owing no man anything. He was one of those who early rebels against paternal authority, because of resentment toward the father and who preserves this attitude of rebellion and independence through all the storms and changes of puberty, adolescence, and manhood. The boy who most rebels against the father's authority is likely to be he who most closely imitates the character of the powerful and despotic father. So Segantini came, in full maturity, [illegible] when his will to dominate, and his realization of se [illegible] to a new sort of artistic expression.

It was now that he went to live as a mountai [illegible] order to study and to paint the marvelous light [illegible] peaks. His artistic aim was to *master* the se [illegible] and light, while a deeper, subconscious, motive led him to climb, and to live, far above the dimness, the pettiness, and the confusion, of the valleys. This is the period of enthusiastic joy in his work and of ecstatic love for nature. His own descriptions of his feeling for natural beauty so closely resemble descriptions of the ecstacy of love that there is little room for doubt that his nature love was a sublimation of sexual love. He was happily married now to the only woman he ever loved, but he had love, in his rich nature, left over for Nature and Art. Of formal religion he would have none, so his worship is given to Nature. The motherhood motive is still present, but it now takes on a fuller, more joyous tone of life and of oneness with Nature. At this time he painted "The Two Mothers," showing a human mother and child close by a cow and her calf. This picture is one of his masterpieces. It breathes rest, peace, harmony, quiet strength, and, like all his pictures, it has a meaning. It insists upon the Oneness of Nature and Humanity, with Motherhood as a keynote of both. Other pictures were the "Pagan Mother," and the "Christian Mother." He painted always with purity and refinement, as is seen in his rare treatment of the nude. At all times he shows his infantile fixation but in all his work and life he shows that he was one of the best of sublimators.

He reached his goal, he mastered color and light, in his own way, independently of the work of other artists, and, this phase of his nature satisfied, he swung back, as neurotics do, to a darker, and an older, phase. Like the neurotic he began to retire into the world of fancy, to paint sadness and sorrow in fanciful and symbolic forms. Now came his so-called Nirvana pictures. Now he paints two strange pictures, one, "The Hell of the Voluptuaries" taken from the Buddhist story that bad women and the wicked mother are doomed to wander upon a trackless frozen waste. The other picture represents a dying mother, with a living rosy babe, swaying from a tree in the twigs of which her hair is caught. The posture defies all the laws of nature, but for this Segantini did not care, nor did he deign to explain the picture, showing how all-important the world of fancy had become for him. The picture represents the punishment of a wicked mother, and the plight of the forsaken child. He was the forsaken child, as he was the child of his Madonna, and as his portrait of himself shows Christ-like features. He is expressing here his long repressed and unconscious resentment to the mother who forsook him, and to whom his feelings were so pronouncedly ambivalent. This strange picture, viewed in the light of his mother complex becomes one of the most significant he ever painted. The sadism aroused when he drew the sketch of the dead child for the weeping mother is re-awakened now, as other activities at this time show.

Following this period comes the period when he painted Death, notably his picture of the "Return Home." The last phase of the complex is now finding expression. At this time he dreams of death, and, being somewhat superstitious, he attaches meaning to the dreams. He was living now high up in an Alpine hut, working with a superhuman energy. He was taken ill and in his fever he got up repeatedly, went out into the bitter air, and worked upon his paintings. He refused a doctor also, and so, finally, he died from the effects of the exposure and over-exertions, since the illness itself was not likely to prove fatal. This feverish activity of his has been generally attributed to his abounding zeal for work, but in view of the death dreams shortly preceding it, in view of the cycle of mental evolution which he was completing, based upon his complex of Motherhood and Death, it seems very likely that this was a case of unconscious suicide. Such cases are not so rare as might be thought. Many a seeming accident is a suicide, *purposive* but *unconscious*. Segantini had accomplished his object, he had lived to paint Nature, Motherhood, Love, and Death. He had conquered his world, and each phase of his great motivating complex dominated him in turn, and the last was death. He expiated the old, fancied wrong of having been the cause of his mother's death. This dark thought had been long repressed but it came into its own at last. "Gladly he lived and gladly died, and laid him down with a Will."

"This psychoanalytic survey which has given us a glimpse of the battle between conscious and unconscious powers, allows us, perhaps, to grasp this inner strife and sympathize with it. It reveals to us the whole tragedy of the great artist so early dead, which was that the shadow of death walked with him, *him*, the untiring creator, all the way." Thus Dr. Abraham concludes his study, and this conclusion is convincing in some sense at least.

IV

ANDREA DEL SARTO'S ART, AND THE INFLUENCE OF HIS WIFE

By ERNEST JONES, *Imago*, Vol. II, 1913, p. 468-480

For many generations of art critics it has been inexplicable why Andrea del Sarto, in spite of his admirable contributions to every line of painting, is yet not an artist of the first rank. The more carefully his work is analysed in detail the greater is the admiration of the beholder and yet more of the connoisseur. His drawing is unexcelled in correctness, he was the best colorist of his time, surpassed in this line only by the Venetian School. He was a past-master of chiaroscuro, his compositions were of an almost perfect harmony, his frescoes show us the highest achievement in that realm of art up to the present. It is not to be wondered at, therefore, that he was called by his own very critical contemporaries "the painter without fault," which is the more significant when we consider that he lived in Florence at the very time that Raphael and Michael Angelo were producing their masterpieces, when the art of the Renaissance reached its highest point. In spite of all this we are confronted with the fact that Andrea del Sarto never reached true greatness in his art, that his work lacked something essential which robbed it of any claim to rank with the first masters.

All the biographers and critics, from Andrea's own time down to the present, are practically agreed on this point and most are agreed in saying that it is because our artist possessed talent in high degree and yet lacked that intangible but essential quality of inspiration or super-ability—call it what you will—that we name Genius. The inspiration of an inner light, a bright ideal, the divine touch, is wanting in him. Robert Browning has given us the most complete and profound psychological analysis of Andrea's strength and weakness yet given, in his poem "Andrea del Sarto." The psychoa [illegible] accept Browning's analysis in full, only further inter [illegible]

> "I can do with my pencil what I know [illegible]
> What I see, what at bottom of my h [illegible]
> I wish for, if I ever wish so deep—
> Do easily, too—when I say, perfectl [illegible]

I do not boast, perhaps.
. . . I do what many dream of all their lives . . .
There burns a truer light of God in them,
In their vexed, beating, stuffed, and stopped up brain,
Heart, or what e'er else, than goes on to prompt
This law-pulsed forthright craftsman's hand of mine.
Their work drops ground-ward, but themselves, I know,
Reach many a time a heaven that's shut to me,
Enter and take their place there, sure enough
Though they come back and cannot tell the world.
My works are nearer heaven, but I sit here. . . .
Ah, but a man's reach should exceed his grasp,
Or what's a heaven for? All is silver grey
Placid and perfect with my art: The worse!"

The poem goes on to show that in addition to, and perhaps as a cause of, this lack of the divine fire, the artist is hampered and pressed down in his work by his mercenary, selfish, domineering wife, whom none the less he slavishly adores.

"But had you—oh, with the same perfect brow,
And perfect eyes, and more than perfect mouth,
And the low voice my soul hears, as a bird
The fowler's pipe, and follows to the snare—
Had you, with these the same, but brought a mind!
Some women do so. Had the mouth there urged
God and the glory! never care for gain,
The present by the future, what is that?
Live for fame, side by side with Agnolo!
Rafael is waiting: up to God, all three!
I might have done it for you. So it seems
Perhaps not. All is as God over-rules."

The poet and the biographers are undoubtedly right in casting much of the blame for Andrea's failure upon his wife. Yet other artists have not needed the inspiration of a noble woman in order to reach the highest achievement, other artists have struggled against every human obstacle and have won. This, too, the poet recognizes, and concludes that the lack was, after all, in the artist, though without the millstone round his neck he might have soared into the upper air. The psychoanalyst would add to the poet's interpretation a further analysis of Andrea's relation to his wife Lucretia. It is known that she was cold, mercenary, without understanding of his art, jealous,—soulless, one would say—a woman who sucked him dry of everything and gave him nothing—except her beauty. She made him desert his own parents, whose only support he was, and take the support of her family upon his hands. She grew restive and jealous when he was in France painting for Francis I, enjoying the opportunity of a life time, and called him back to her, regardless of the dishonorableness of breaking his contract and leaving unfinished the work for which he had already been paid. Because of her tyranny he lived in poverty and dishonor painting with always more facility and perfection of technique and always less of spirit and meaning, until he became, as he calls himself, nothing more than a craftsman. Andrea submitted to her and adored her to the last, yet he would be more than human did he not feel and resent all this, especially as he very well knew that without her throttling hands about his throat he might have aattained the honor and distinction won by Rafael, Angelo, and Leonardo. He loved her, it is true, but the under side of his love was hate, and it was strong in proportion to his love. His feelings were *ambivalent,* and hence arose a bitter conflict because Lucrezia was always there. Had she been unfaithful to him or had she died, he might have, from the conflict involved in his resentment, sorrow, or pain, released his pent-up emotion in great works, comparable to, or surpassing, the works of Leonardo da Vinci. But he could not get away from her, and moreover he loved her as a woman loves a man. He was naturally bent toward homosexuality, and this presents another ground of conflict. She kept him from his man friends in addition to all the other injuries. He was her willing captive, of course. Quoting Browning again, we hear:

> "So—still they overcome,
> Because there's still Lucrezia—as I choose."

Yes, he consciously chose her but unconsciously he hated her and rebelled against her tyranny. So the ever present conflict of every day of life absorbed his strength. It could not even be pushed back to the original infantile setting—which in Andrea's case we know nothing about—from thence to find a new outlet, as Leonardo's did, because Lucrezia remained the all-absorbing motive of his life. This brings us back again to the question, "Was it her fault after all, when all is said and done? Was there not some fundamental lack in the character of a man who could so submit to the stifling, crushing influence of such a woman?" and again the psychoanalyst confirms the poet's verdict. We do not know anything of significance about Andrea del Sarto's early life and development, so we cannot find the first beginnings of his defects, as it is the business of psychoanalysis to do, but we can say with a measure of confidence that early influences had so shaped his character as to make him a ready victim to any domineering woman who could please his artist's eye. Such was his weakness, his masochistic tendency, that he must have submitted to some one, and had it not been Lucrezia del Fede, it would have been another like her. This does not exonerate her, but it places some of the responsibility upon the poet's own psychological constitution. It was not lack of ability that kept him down, however, it was a tangled and wrongly directed complex of emotions which centered about this wife of his. Could he have lived in a world of men, of artists, alone, he might have found a worthy self-expression. Or if he could have been two, as Browning makes him say, if he could have been a larger, broader, personality, so that he could have reacted otherwise upon his daily difficulties, then he might have shown the genius of the creator, instead of the mere talent of the craftsman. He might have been an artist, whereas he was only a painter.

V

THE PROBLEM OF HAMLET AND THE "OEDIPUS-COMPLEX"

By ERNEST JONES. (*American Journal of Psychology,* Vol. XXI, 1910, 72-113. Translated into German by Paul Tausig, Wien, 1911. The references in the abstract given below are to the German edition).

The problem of "Hamlet" has been called the "modern Sphinx," and an endless array of chapters, and whole books [illegible]oted to the attempt to unravel the mystery of why [illegible] not perform the task that he so clearly saw to be his duty. The claim of this drama upon our interest rests not only upon its mystery, however, but yet more upon its high artistic and literary excellence. It is on a higher level than any of Shakespeare's other works, and is taken commonly to represent the greatest achievement of his genius. Although Shakespeare was the least subjective of poets we may fairly assume that in Hamlet we have something of himself—perhaps the core of his philosophy, for no man could touch such spiritual heights and depths without putting his own soul into the written word. If we could solve the mystery of Hamlet, therefore, we should have a new insight into the character and genius of Hamlet's creator.

The explanations that have been put forth hitherto are opposed to the context of the play itself. Hamlet was *not* a contemplative spirit, incapable of decisive action, as Goethe suggests. He showed himself a man of quick and decisive action in his dealings with the Ghost, with Polonius, with Rosencrantz and Guildenstern, and with Laertes. It was only in the one case of his revenge upon his uncle that he could not act. The task was *not* rendered impossible by external circumstances. The reader, or the spectator, is clearly given to understand that Hamlet was the idol of the people and would have had their support in anything he did. Nor was there a moral conflict between the task of revenge imposed upon him and his Christian feeling of repugnance to human vengeance. There is no indication of such a conflict in his soliloquies, though there is unmistakable evidence of a mental conflict which was not fully conscious. Such a moral conflict as the above would have been inevitably conscious. The solution is not that he *would* do the deed but *cannot*, nor that he *could*, but *will not*, it is that he *cannot will*, because there is a repugnance to the task that he himself cannot understand because he is unaware of its source. He knows his duty yet he keeps finding excuses, like a reluctant school-boy, always trivial excuses, always different excuses. Who of us does not know that when we give ever-changing reasons for a course of action, each one of which we realize to be inadequate, the real reason is one which we will not acknowledge or one which we do not know? We only know that we have a compelling impulse. In Hamlet's case there is deep remorse for his conduct, and his sense of guilt is easily aroused. He is deeply depressed, his attitude towards the world and towards life is one of tragic hopelessness, yet he has a dread of death. His repeated reference to bad dreams, his self accusations, his desperate efforts to get away from the thoughts of his duty, and his vain attempts to find an excuse for his recalcitrancy, all point to a tortured conscience, to some hidden ground for shirking his task, a ground which he cannot or dare not avow to himself. (p. 21.) This state of feeling surely points to an unconscious conflict, which we may try to investigate and explain by means of psychoanalysis. In looking through the play we find abundant evidence, in Hamlet's relations to various characters, that that strongest of repressed complexes, the Oedipus-Complex of infantile sexuality, played its part in Hamlet's life.

His love for his mother is strong and ardent, although it is mixed with condemnation and horror, because of her guilty union with the king. His greatest horror is for the incest of his uncle and his mother, and not for the murder of his father by the uncle. (p. 45.) The physical loathing he felt for this is a common symptom of sexual conflict.) It was not mere jealousy of his mother's affection that made him so resent her marriage, nor mere respect for convention or for his father's memory. It was a projection of his own guilty thought, of the suppressed and unconscious desire, born in childhood, to possess his mother for himself. When his father died the repressed wish stirred and woke, though it did not emerge into consciousness. His attachment to Ophelia (p. 42) was perhaps caused by his desire to play her off against his mother, since she was the opposite type of woman. His coarse jests with Ophelia and most of his attentions to her also were carried out under the eyes of his mother. Everyone who knows children knows how the youngest of them play one person off against another, with infantile jealousy and coquetry.

The second marriage of his mother aroused his buried wish concerning her yet more effectually and the marriage with the usurper was condemned as incest with an access of loathing due to his subconscious incestuous desire. The conflict of these struggling unconscious desires with his rational morals wrecked him as it has wrecked many a neurasthenic known to us to-day. An indication of how *near* to consciousness his guilty wish came is found in his cry "O my prophetic soul, my Uncle!" when the ghost informed him of the murderer. His first reaction after this revelation is against Ophelia, revealing the sexual nature of the conflict. It is both a reaction against women in general and against her hypocritical prudery. (p. 43.) His sexual feelings are now being powerfully repressed, in the effort to repress that horrible, struggling, unknown desire. But losing Ophelia only increases the conflict with the repressed mother love. "His detestation of his uncle is the jealous detestation of one evil-doer towards his successful fellow." (p. 44.) Therefore he cannot denounce him freely without bringing his own guilt to consciousness. His moral fate is bound up with his uncle's. The call of duty to slay his uncle cannot be obeyed because it links itself with the call of nature to slay his mother's husband, whether first or second husband.) The latter call is strongly repressed and therefore the former is also repressed. Hamlet spoke more truly than he knew when he cried out, "I am prompted to the deed by heaven and hell!"

There are secondary elaborations of the father-son complex in the Polonius and Laertes parts of the plot. The tedious and futile character of Polonius and Hamlet's contemptuous treatment of him are disguised presentations of filial resentment toward the hampering and pestiferous rivalry and control of the aged parent or friend. Hamlet satisfied the requirement of the popular myth, which is but a race-dream, by slaying Polonius, since he could not slay his own father. The various sagas on the Oedipus theme show in different guises the tyrant father. Sometimes it is a great-uncle, as in the Romulus myth, sometimes a grandfather, who opposed the marriage of the parents (a father-daughter complex, the complement of the Oedipus complex, enters here). Here there is a doubling of the parts and a division of the characteristics between the two persons. Polonius represents not the tyrannical side of the father, but the interfering, tiresome, superannuated person, whose main offence is that he is in the way.

The story of Hamlet was a well known saga, which Shakespeare took up, altered, and adapted to his purpose, according to his custom. The significance of the drama for Shakespeare's own personality and history does not lie in his selection of this particular story—

for he could scarcely have missed choosing one with a similar theme—but in the *alterations* which he made in it. In most popular versions of the saga Hamlet takes his vengeance after overcoming numerous external difficulties. Why did Shakespeare make the difficulty a subjective one and the conflict a psychological battle veiled in mystery? Why, unless in hearing the original story his heart was stirred with the feeling of kinship, with a sense that *he* had been in a similar spiritual situation and a realization that he would not have met it as Hamlet of the saga did? How could he so clearly depict an unconscious conflict unless he had felt such a one in his own soul? It is a suggestive fact that the drama of Hamlet appeared the year after Shakespeare's father died (1602). Could this event have waked the sleeping complex in the poet's soul, causing him to write that which he knew not fully—like Jensen in his Gradiva—and to express thus the pain and grief that oppressed him without revealing their source? The play of Hamlet is the essence of Greek Tragedy in that it depicts the desperate, unavailing, struggle against Fate. The Fate is Hamlet's own Will to Death, which is so much stronger that his Will to Life that it makes all his efforts to overcome his difficulties involve him only in deeper mire. It is Fate, in that the Will to Death is an unconscious force, completely beyond the control of his conscious will. (p. 64.)

Whether or not this be the true explanation of the Hamlet mystery, it more nearly satisfies the requirements than any other that has been put forth. If the psychoanalysts are right in their description of the "Oedipus-Complex" as "fundamental and universal it is only fitting that the greatest work of the world-poet should have been concerned with the deepest problem and the intensest conflict that has occupied the mind of man, with the revolt of Youth and of Love against the fetters which jealous Age imposes upon them." (p. 65.)

VI

ON DANTE'S UNCONSCIOUS SOUL-LIFE

By ALICE SPERBER, *Imago,* 1914, Vol. III

This study does not pretend to be exhaustive, nor does it attempt to explain Dante's genius. Its aim is rather to throw some light upon his personality and upon his career as a poet. It leaves room for much further study of the same theme.

"The problem of Dante's nature and of his creations consists in the co-existence of bold revolt and humble obedience, of admirable new thinking and slavish belief in authority. The same man who hurled wild curses at the Papacy was the truest son of the Church. The poet who leaves no heart untouched could never free himself from the cold formalism of the scholastics. The scholar who was the first to dare to expound scientific problems in the Italian instead of in the Latin tongue—and also the first to solve in so spirited a manner the complicated problem of Italian written language—restrains his boldest flights of thought and rests upon the authority of Aristotle and the Bible. How comes it that this universal, genial, and courageous spirit languished, as if through a spell, in the chains of authoritative dogmas, while Petrarch, who was certainly not so courageous, dared in the fourteenth century, to assert that Aristotle was not the court of last appeal for Science, and Leonardo da Vinci, in the fifteenth, launched boldly forth on scientific investigations while yet the authority of the Church was supreme?" (p. 248.)

If we look for the answer to these questions to the soul-life of the poet and to his childish experiences, we shall find, in his writings and history, material at least as valuable as that we found for Leonardo da Vinci. If Leonardo revolted against authority because of the animus against his father, Dante clung to authority because of a love and reverence for his parents and a conflict that had made him find gratification in submission. He lost his mother in his early childhood, but just how early is not known. His many references to mother-love and mother ways make it seem certain that he dimly remembered a mother, whose memory was enhanced by the very dim, remote, uncertainty of his hold upon it. For instance, he speaks of "a mother comforting her frightened child," and a "child who hangs his head in conscious guilt before a mother." It is the nature of such references as these that gives us the needed clue.

The most significant feature of his life, apparently, and for his poetic works—and therefore for his emotional experience—was his relation to Beatrice. So disproportionate is the extent of his actual acquaintance with her to the rôle which she plays in his writings that some critics do not hesitate to say that she had no real personal existence, but was an abstract creation of Dante's fancy, and that she symbolizes philosophy. Boccaccio, however, relates her history and her connection with Dante in full. Whether she was real or ideal is, after all, of less importance to our problem than the determination of what she meant to Dante. There can be no doubt but that she was idealized. In her we may discern the nature of his ideal of perfect womanhood.

In the Divine Comedy she is again and again compared to a mother, while the poet compares himself to a fearful, guilty, or shamed child. Beatrice adopts a stern and rebuking demeanor toward him, for his soul's good. (p. 210.) Dante feels himself humbled and "dissolved in tears" when she reproaches him for having given his love to other women, or when she smiles at his childish reasoning. He is at once humbled and comforted, and he delights to represent Beatrice in the character of an authoritative and commanding parent. These passages give the clearest possible indications of masochistic tendencies, that is, the desire to experience pain or punishment at the hands of the beloved one. It is very evident that the quality most valued in Beatrice was her firmness, her masculine trait. In one passage he compares her to an admiral assembling his men for battle, and in another to an eagle.

On the other hand he looks upon his guide in the Inferno and Purgatory, Vergil, as a father, and describes him as all mildness and tenderness. (240.) He sometimes compares him also to a mother. What history tells of Dante's father indicates that he was a man of little force. Perhaps Dante idealized his weakness, or perhaps he was really the sort of father that Dante pictures in Vergil. He died in the poet's eighteenth year. At any rate it appears, from the characterizations of Beatrice and Vergil, that the positions of the two parents were somewhat reversed. His love for the mother and for her representative, Beatrice, was of the submissive sort, not the aggressive, and his attitude toward his father was more that of a son to a mother.

In the Vita Nuova, written in his younger days, Beatrice is invested with only a few motherly traits. This work is like the works of the minnesingers and troubadours in its extravagant exaltation of the lady and the abasement of the lover, and cannot therefore be taken exclusively as an expression of Dante's own personality but should be taken also as an expression of a dominant note of the age. But a point of interest, for our study, in the Vita Nuova, is his repeated presentiments, fears, and dreams of the death of Beatrice. If such dreams are the fruit of unconscious wishes, then Dante, perhaps, expresses feelings here of which he did not know the spring. In his grief and abasement at his lady's slighting treatment he may have wished her dead and so in his power, but back of this lay deeper wishes. His mother is dead and Beatrice is identified with her. His mother's sternness no doubt aroused revolt, when he was a very little child, and the infantile wish for her death followed. The death wish is very much in keeping with the masochistic complex, as psychoanalyses of neurotics (219) have shown. His play upon the number nine in connection with Beatrice (217) may mean another link in his unconscious identification of her with his mother. He met her when she was beginning her ninth year and he was closing his. They met again after nine years. She died upon the ninth day of the ninth month. He himself connects the number symbolically with the Trinity but the psychoanalyst would say that the whole complex might have sprung from the unconscious association with the nine months of pregnancy, in motherhood.

The character of Dante's parents and his relation to both, as indicated by the material in his writings, may now be pointed out as the answer to the question: "Why did his bold and brilliant intellect remain shackled in medieval orthodoxy?" The mild and non-interfering, yet reverenced father could arouse no revolt, and as respect for authority usually grows from or exists with respect for parental authority, Dante had no impulse to emancipate himself. The dominance of the mother held him yet more, especially as she was idealized as a Saint in heaven, whom he hoped to join there. Beatrice, dying early also, strengthened this lure of the other world, and helped to keep his feet in the path that was supposed to lead thither. The bonds of affection formed in early childhood are stronger than any voice of Reason.

In conclusion we will take up a significant dream recorded in the ninth canto of the Purgatorio. He dreamed that an eagle came down, seized him, and carried him upward as far as the zone of fire which separates earth from heaven. He thought the place from which he was taken was the very place from which Ganymede had been snatched away, and that this therefore was the eagle's usual hunting place. When he reached the fire it burned him and he awoke. In his waking he compares himself to Achilles, who wakes thus, ignorant of his whereabouts, when his mother, Thetis, carried him to Scyros in order to keep him from the Trojan War, and, further to insure his safety dressed him as a girl. We have seen the mother symbolism of the vulture in the Leonardo analysis. The eagle has a similar significance. Long study of the fire symbol in dreams and in popular speech, has brought its sexual significance to light. Entering the fire here represents the culmination of sexual life—the entry into paradise—and Dante is only like a million other dreamers whom the Censor wakes at this point because the dream becomes unbearable.

The Ganymede and Achilles elements are significant. Both had mortal fathers and nymphs or goddesses for mothers. (235.) Dante's mother became to him an immortal, an angel, while yet he was very young. The strength and beauty of Achilles and Ganymede represent manly ideals also, leading to his identification of himself with them. Both the Ganymede and Achilles myths are homosexual. This does not indicate homosexuality in Dante, but bisexuality, as his historical character also shows. It is generally believed that a high degree of bisexuality is always present in the artistic temperament. The feminine qualities of Shakespeare have been noted by students of literature.

To return to the eagle again, it is elsewhere used as a symbol of power and is also closely associated with Beatrice. In one passage she is represented as looking straight at the sun, like an eagle, and while he was with her Dante *found himself able to do this also.* (237.) St. Augustine has said that only those young eaglets that can look upon the sun are acknowledged as Sons of Eagles, while the others are thrown from the eyrie. So Dante, knowing this passage perhaps, through another thread of unconscious association makes himself the Son of the Eagle. His dream of being carried thus upward represents also a wish for death, a revival, mayhap, of the old childish wish that his angel mother would come down and take him up with her. So Achilles, destined for early death, was placed by his mother on the isle of Seuke, identified by Pliny as the Isle of Souls.

That the mental and spiritual development of Dante was strongly determined by his father and mother in their relations to each other and to him—these relations being unusual, is almost beyond doubt. That he owes to the parent-complex some measure of his poetic fame also is our claim. Beatrice is clearly seen to be his great inspiration, but behind Beatrice stands her prototype, the mother. As both these objects of his love were withdrawn early from earthly contact, his fancy was drawn after them to another world where he hoped to find them again. Hence the longing and the divine discontent that caused him to explore the regions of the soul, and to write in deathless numbers the experiences of his soul on its journey.

VII

The Love-Life of Nicolaus Lenau

By J. Sadger, 1909, *Schriften VI,* 98 p.

Nicolaus Lenau, the great Austrian lyric-poet, presents to us a problem of life and character as well as of genius peculiarly fascinating to the clinician. That he was neurotic in disposition is apparent to the layman. His temperamental oddities, his passionate individualism, his piteous end in the darkness of paralysis and manic-depressive insanity, are enough to indicate that there is material for pathological analysis. For the psychoanalyst there is added his intellectual and emotional attitude, called *Welt-Schmerz,* his celibacy, and his unusual love affair with Sophie Löwenthal. About this latter feature of his life this study is centered, as it includes or touches upon all the significant events or tendencies that are needed for a psychoanalytical explanation of Lenau's unusual character, and the expression given to his genius.

In investigating this great passion of his life we ask, and attempt to answer, three questions: First, What really were Lenau's relations to Sophie Löwenthal? Second, Did these relations cause in any way his final illness and mental derangement? Third, How did they influence his creative work, his character, and his spiritual development?

From the passionate love letters—some of the most beautiful in the German language,—as well as from what is known of Lenau's intercourse with Sophie, we know that they were lovers in everything but the breaking of the woman's marriage bond. Sophie was the wife of Lenau's friend, Max Löwenthal, who never, apparently, objected to the affair, which extended over years, although Lenau treated Sophie as a lover would, in every way. One questions, How could such a relation go so far without going farther? The received answer is that the lady's virtue and faith were unassailable but this is scarcely consistent with the freedom with which she yielded herself to her lover in everything but breaking her marriage vow. The psychoanalyst finds the solution of this inconsistency in the fact, well supported by statements in her diary and letters, that she was sexually anaesthetic. This was one of the symptoms of the hysteria which manifested itself in various ailments and moods, and which, of course, was conditioned by infantile complexes. In this brief summary there is not space for an analysis of Sophie's character, which Sadger has admirably carried out. Suffice it to say that her attraction to Lenau was conditioned by features of resemblance that he bore to her father and that her neurotic disposition, *plus* her anaesthesia, were potent factors in holding him to her through the years, although she gave him fully as much pain as pleasure.

Lenau had much in his childhood experience to prevent a normal development. His heredity was poor, his father died when the boy was only five, his mother idolized him above everything else in the world and indulged him boundlessly. The effects of her over-indulgence and of the unwholesome habits she allowed him to form were apparent throughout his life and notorious in his circle. She "waited on him hand and foot" as the vernacular has it, and pampered his body without stint. The result of all this was that he preserved his infantile mother-fixation just as Sophie preserved her father-fixation, and therefore each fulfilled the other's ideal, and they loved each other passionately. They never found it too hard to keep Sophie's marriage vow intact just because they loved in an infantile way. Each received from the other just what he and she had been accustomed to receive from mother and father—and no more.

Not less important than the mother's influence was that of Lenau's father, in a different way. The latter died, of tuberculosis, when Nicolaus was five years old. His only memory of his father—as he relates in after-life—was that of the tall, sick, white-clothed man, rising up and giving him a box on the ear because he was too noisy. It was a memory that rankled. On his mother's account his feeling toward his father was ambivalent, and this incident enhanced the antagonistic attitude, which gathered round it and crystallized his emotions into fear and hatred. It was soon after the father's death that the six year old child wept for half a day because he must sometime die In after years this experience came out in his morbid fears and fancies about Sophie's death—he had them most when his longing for wife and children was greatest, and so, in spite of his love for her there was an unconscious, or possibly conscious, rebellion against her sway. His childish terror at the thought of death the psychoanalyst knows to have been the result of the fulfillment of his death wish against his father and fears were roused about Sophie on a similar ground. This incident gave rise to another significant mental state in later life. He more than once confessed that he always feared a "stroke" and when, near the end, a stroke of paralysis came to him he recognized it as the fulfillment of his presentiment. Sophie wrote, just after this, a consoling letter in which she innocently quoted the words:

> "Duck, and let it go over you,
> The storm must have its will."

He vehemently crossed out the word "Duck" and wrote "I will NOT duck!!!!" reiterating the denial on subsequent occasions. A complex was obviously touched, the complex that started in the incident of his father's blow, the nucleus of a mass of fears and resentments.

Three times Lenau fell in love and hoped to be married, and the first and second times the infantile fixation upon his mother blocked the move. His second love, Caroline Unger, was a woman who demanded service of him instead of becoming his slave, as all other women had done. Perhaps we do not need to trace his break with her back to his filial complex, but at any rate she was very different in this from his mother, which is some grist for the psychoanalytic mill. After he knew Sophie Löwenthal she was a factor in keeping him from marriage. She could never bring herself to encourage him, though she knew she could not fully satisfy or make him happy. This made her feel more guilty, in her heart, than if she had actually transgressed the laws of society for his sake. He, on the other hand, was ambivalent toward her because of her failure, as is shown in his attitude after his insanity developed. His attitude toward her was that of a child to his mother. (76.) He loved her passionately, yet he felt the restraint she exercised over him, and he longed for marriage and children, which, he once declared, were the only realities. Toward the close of his life he became actually betrothed to Marie Behrens much to Sophie's disgust, but his illness and insanity intervened, so that marriage was never consummated. During this time he was obsessed with the fear of Sophie's death, meaning, as we have seen, that he subconsciously wished her out of the way. After the insanity developed he feared to receive her letters, but after he had them he covered them with kisses, showing how he both loved and hated her—as he had loved and hated both his parents.

Now we may give the formal answer to the three questions propounded at the beginning of this study. We have sufficiently described the real relationship of Lenau and Sophie Löwenthal. They were lovers but they never sinned against the social law. As for the second question we may say that in spite of Sophie's influence, not by any means all for good, the paralysis and insanity are not to be laid at her door. They were directly caused by an attack of syphilis he had following an adventure at Bremen. But the form of his delusions, and possibly the moment of the inception of the mental disturbance were determined by his relations with Sophie, and these, pushing further back, were the results of the infantile sexual fixations of both Nicolaus and Sophie.

As to the third question—what relation had her personality to his creative work and his spiritual development, we may say that her influence was next in power to his mother's. She was the inspiration of his *Liebesklängen,* the loveliest of his poems. She furnished an object for his love—for his libido—and yet denied him full satisfaction, so to her influence must be attributed a part of the *Welt Schmerz* that pervades his work—the feeling of something lacking. Of course, we reiterate, this is further traceable to his mother's character and to the conduct on her part that fostered his selfish individualism. But for Sophie, however, he might have outgrown his infantile fixations more completely.

VIII

HEINRICH VON KLEIST

(A pathographic-psychological study), by J. SADGER, *Grenzfragen des Nerven und Seelenlebens,* 1910, p. 5-63

The life of a poet is here described to whom the good gods granted in his cradle a rich and powerful talent, but to whom they denied that essential to happiness, moderation—self-control.

Heinrich von Kleist was the fifth of his father's seven children. The father was a Prussian officer; the mother, we learn by chance from a letter, had that intense sensibility and instability that were inherited by her son, and that is all we know of the parents. For a clue to his heredity we must turn to the indirect evidence of more distant relatives. Ulrike, his half-sister on the father's side, was a born nomad, possessed with an irresistible *wanderlust.* When she could not actually travel it was her joy to take "map-journeys." In mature years she developed traits that point to senile dementia. Kleist's cousin on his mother's side early became the victim of melancholia, and committed suicide. These facts make it probable that Kleist had a poor heredity on both sides. The expected stigmata in his case would be: chronic depression and longing for death, the most intense happiness in love for a short time, which is involved in the incapacity to bring his ego into harmony with anything else, and, finally, boundless extravagance in every direction. Does he exhibit these? We have the testimony of Zschokke and Bülow that he suffered from melancholy from earliest youth. (p. 6.)

The second stigma, the hereditary incapacity to "fit in," is certainly present. It is shown by his *wanderlust,* his inability to adapt himself to regular duties, regular responsibility, to regular relations of any sort, or to adopt a settled profession. His wanderings are attempts to escape the unhappiness in his soul. Every new trouble is the stimulus to a journey, with what end he knows not; the only purpose is to flee. The same opposition to settled relationships is shown in his affairs with women. In the space of fourteen years nine had caught his fancy, but none had made a really deep impression. The engagement with Wilhelmine was broken off entirely by his fault, because of his neglect and inconstancy, and it was well indeed that it never culminated in marriage. He could have made no woman happy.

The same inconstant character showed in his attempts to find a vocation. He would have liked to be everything! He was successively soldier, student, teacher, volunteer in the Technical Deputation, poet, state-employee, newspaper-publisher and editor, besides many other callings of which he merely dreamed, but which he never really attempted. In one of his letters he says, "I feel myself altogether incapable of placing myself in any conventional relationship with the world." If anything could attract him and hold him it was, as is so frequently the case with the hereditary neurotic, an academic career, with its freedom of learning, or else the career of a writer, since both of these allow frequent change of objective. For, stronger than his native capacities, mightier than his genius, greater than his poetic ability, was his opposition to subjecting himself to fixed routine. He once remarked significantly, "There is nothing consistent in me, except inconsistency!"

The immoderateness that characterized his sensations, his efforts, his actions, may well have had its cause in this same instability. At the University he enrolled for so many studies that the Professors themselves thought he had taken too much, and he declared "If I manage to carry it, then I can with right assert that I have made the impossible possible." His ambition knew no bounds, nor did he limit his demands upon others. His friends truly saw that this ambition was pathological and he himself glimpsed the truth. He knew that if he did not reach his goal quickly, by an extreme effort, he would never reach it. He knew his time was short, his power limited. "Hell gave me this half-talent," he wrote. "Heaven gives to mankind either a whole or else none."

We have abundantly seen that von Kleist had the stigmata deduced from his heredity. We shall see further on that these traits have also a psycho-sexual explanation, and that this fact is not in contradiction with their hereditary character. To the inborn nervous instability is added a series of experiences which gives to his character its special forms and colors. The first of these psychic components is homosexuality. We do not mean to say that Kleist was guilty of any gross expressions of this, and we do not mean to say that in this he was worse than other men. There is an epoch in the life of every man and woman when he or she is most strongly attracted to the like sex, and this epoch is normally before maturity, or, sometimes, in old age. It is by far most frequent and most violent in puberty, as we all well know. In the neurotic and the mentally diseased this normal phase is exaggerated and prolonged, and so it may be also in the poet and the artist. Letters of Kleist are extant to prove that he loved at least one friend with the fervor of sexual passion, and he almost recognized its character himself (p. 14), although he must hold to the name of "friendship" and deny that of "love" to his passion. Such a love did he give to his teacher Martini, and the influence that Martini had over him was vital for more than ten years. Martini imparted to him a thirst for knowledge. He rushed, like a true *Schwer-Belasteter*, into remotest realms of learning. Even while in military service he was more of a student than a soldier. An outcome of his love for other friends was the youthful sin of masturbation, which he afterward turned from with bitter self reproach.

But in the years of puberty love for the other sex developed also, that is, he was bi-sexual. After his first disappointment here he sought refuge in philosophy, as so many neurotics do, as a refuge from the actual and the sensual. Many turn to pure mathematics, as the most abstract of studies, and Kleist himself became absorbed in Geometry. Then he went to Frankfort to study science, with the secret hope, also, of finding another teacher for his heart. Besides, his beloved half-sister Ulrike was there, the dearest relative he had. She too was homosexual, and as masculine as the poet was feminine. She was the only person who thoroughly understood him. While at Frankfort he wrote much on the theme of Virtue as the only road to happiness. Clinical experience leads Sadger to think that this youthful pre-occupation with and worship of virtue is always an over-compensation for the childhood sin of masturbation. His longing for trust and confidence, his need of confiding in his friends and being understood by them, his passionate desire to have no secrets among friends, to be perfectly frank and open, is also an over-compensation for the secrecy of that indulgence. The love for abstract studies, as "pure" mathematics, is another such compensation. A further symptom in his blushing, embarrassment, and stuttering in company.

A further instance of his homosexual attachment to the teacher Martini is his very evident imitation of him in the didactic letters he writes to Wilhelmine, his betrothed. These letters are rather strange in another particular, coming from a twenty-three year old lover. He talks to Wilhelmine of coming motherhood, of her high destiny as mother of his children, he tells her that he longs to mould her into such a wife and especially such a mother as he could wish. He does not love her for herself indeed, but only as the material out of which he can form a woman to suit him. He pictures her in fancy with two children at her feet and one in her lap. As he had two younger brothers and sisters this might well be a reproduction of one of the earliest experiences of his childhood, a picture of his mother with her three youngest children, one of whom is himself. We know now, says Sadger, that homosexuality springs from the unconscious persistence of the boy's first love—his love for his mother. The homosexual impulse is a turning to the womanly attributes in men because other women are rivals to the mother. When he seeks women he seeks those of whom his mother is the prototype. He really seeks her. His mother died when he was very young. His erotic life took two directions, homosexuality and masturbation.

In Kleist's relation to his fiancée we get a significant diaresis, the Würzburg Journey, which has been a puzzle to all the biographers. Of the many solutions offered it is probable that all have a grain of truth, that the truth is a combination of the various motives assigned, for we have come to realize that few psychological motives are simple. It was the poetic call, and not only that but the result of depression of spirits and the fear of psychical impotence, because of a congenital anomaly, which led him to seek advice of a physician, although this fear, awakened by his correspondence with Wilhelmine concerning their approaching marriage, was undoubtedly the occasion of it. He did not go to a renowned physician, however, but to a "wise, noble, and old friend," Brockes, to whom he made a general confession and on whom he depended for help and advice. From Brockes he receives both help and comfort. With self-sacrificing zeal this friend stood by him "like a mother!", so Kleist's account runs. He succeeds Martini in the heart of Kleist and gains over the poet an equally strong influence, an influence which is indicated by a significant change in the poet's ideal at this time. It is no longer "to know" but "to do," that he aspires, to lose himself in selfless service. This ideal has always found its highest expression in motherhood, and Kleist is inspired by the influence of that friend who helped, guided, reproved, comforted him in a true motherly fashion. This points us back to the fundamental principle that homosexuality is the result of an imperfectly sublimated mother-fixation.

It is at this point of the analysis that the history of Kleist's infancy and of his relation to his parents is most needed, and, unfortunately there are no facts at our command . If we be permitted to reason by analogy, and from result to cause, we may say that Kleist must have had the commonly found worship of his mother, and jealousy of his brothers, sisters, and father. We deduce this from his ideals of motherhood, from his homosexual friendships, and from his unreasonable jealousy of his fiancée. There is evidence also in his poetic works, though such must always be taken with a grain of salt, because it is so difficult to distinguish here between the products of the conscious and of the unconscious. His cherished dream of "a little hut in a valley," where he and his bride should live an idyllic life as peasants and tillers of the soil, is like the child's day dream of taking his darling mother away from all other troublesome encroachers, and having her all to himself in a fairy-like, simple, rosy, existence. On this rock his engagement was shattered, but it was for deeper underlying causes of which this was an overt expression—namely, that he could not surrender his heart to Wilhelmine as he should, that he could not adjust himself to the realities of life, and that he asked of her the impossible. He sought her not for herself, but as the half material embodiment of a dream—and the substance of that dream was his mother.

The most important substitute for his mother that he had in his whole life was his half-sister Ulrike, but even she did not give him the sympathy, the understanding, the self-abnegating love that he demanded, as his letters to her and to Wilhelmine show. She was not a whole mother to him.

Traces of the resentful attitude that we hypothesize as existing toward his father are found in the play "Der Zerbrochener Krug" in the rôle of the Judge, and in his "Prince of Hamburg," in that of the king.

We have noted the fact that any trial or disappointment in the life of Kleist was usually the occasion for a journey, and have seen that this journey was not only to satisfy his resistance against all the then-state of things, but also had deep-going psychosexual roots. Now the circumstances under which these journeys were taken, and his reactions to them give a key to the nature of this psychosexual root, which, it seems, is the unconscious desire to fulfill that old childhood's wish to go far away with mother, far from all troubling people and things—which, in childhood, means all disagreeable circumstances of family life—and to have her, and all her loving care, to himself. We find that he always chose a companion for his journeys; he never went alone. Furthermore, this companion was invariably a male friend whom he loved homosexually. Only one exception is found to this rule—the journey to Paris with Ulrike and this was not by choice but in fulfillment of an old promise, and the journey was not a happy one. As a third point in favor of our theory, the only journey that ever did leave him satisfied and happy was the one he took with Brockes—Brockes, who most fully satisfied his craving for "mothering." The end of every other journey, although it was taken with a well-loved friend, was a violent rupture, a flight on the part of the poet, and suicidal thoughts, sometimes threats, as a consequence of his disappointment. Whatever the ostensible cause of his despair the real underlying cause, all unconscious to himself, was his disappointment at not getting what he unconsciously sought, the satisfaction of motherly love and cherishing. He sets out with a friend, in a seventh heaven of joy, but soon come reproaches for indifference, jealousy, quarrels, a rupture. This proceeding reminds one strongly of the relations between two supersensitive married people, such as Heinrich von Kleist's parents were. He but acted out with the chosen friend, to whom he had transferred his mother-love, what had been enacted before him in childhood. (There is no need of any such explanation, however. Many people coquette in this fashion whose parents never quarreled in their lives. A much more rational explanation is possible.)

The ambition of Kleist, seeming to us now extravagant and presumptuous indeed in its boast that it would "snatch the wreath from Goethe's brow," is an outgrowth of the ambivalent feeling toward his father, transferred afterward, by the widening spiral of spiritual growth, to other masters, elders, and superiors. This ambivalence—alternation of love and hate—shows in his affection toward Wieland the poet and Hindenburg the mathematician, whom he afterward hated, and in his rivalry with Goethe also. The boy wishes to imitate and then to excel his father, to be his equal first, then his master. But in Kleist's case an unattainable goal was set, and he was not one fitted to strive against odds. It has held true in the experience of Freud and his school that those who are aggressive in sex-life are aggressive in the pursuits of ambition also, while those who seek satisfaction in masturbation shrink from hard struggle in pursuit of an aim. Kleist felt his failure very bitterly. He never wrote anything of his great tragedy in finished form, and his inability to accomplish his purpose drove him to despair and suicide. Not this despair alone, but the despair also of finding satisfaction in any love, whether of his own or the other sex, filled the cup of bitterness to overflowing and made him seek refuge in death.

Whether his disease was hysteria or dementia praecox is a little hard to say as it presents features of both. The rising of unconscious thoughts to the surface, interrupting the current of conscious life, which is seen in both these maladies, is shown in Kleist by his extreme suggestibility, by his speaking to himself, between his teeth, and by fits of madness, in one of which he suddenly declared that he must throw a friend into the river in order to possess the friend's wife. When this friend, Adam Müller, came up he actually attempted to throw him in. As he had never evinced any passion for the lady in question, Sadger thinks that the Müllers were mere lay figures, substituting for the actual father that the child Heinrich, still alive in the unconscious mind of Heinrich von Kleist, would kill, and for the actual mother that he would possess. The substitution was the work of the Censor.

Another evidence of the uncontrollable Unconscious was his inability at times to distinguish between fact and fantasy. A notable instance is the account of a midnight feast in Paris, which he wrote of to his sister, and which never occurred. The analysis of this story reveals the hidden wish, and Kleist, like imaginative children, so realistically pictured the fulfillment of his wish that for him it became inseparable from reality. It is comparable to the hallucinations of the insane.

Of Kleist's later life it is needless to go into detailed account. He gave himself up to the care of his sister, himself a victim of hypochondria. From this state the impending Franco-Prussian war aroused him. The need of his country—which always vicariates for father and mother—enabled him to find an outlet for the pent-up desires without resorting to sickness. It was not patriotism that inspired him so much as hostility and resentment toward the enemy, not love, that is, but hate of a powerful invader of his rights—a hostile father. After this he sought his release at last in death, and his suicide has more meaning in it than that of despair because of failure. It is really an erotic satisfaction. He had long nourished the fancy of dying, dying with a beloved person whom he could possess fully, in death. He never thought of dying *alone* and he condemned such suicide. When Henrietta Vogel consented to die with him he was satisfied at last, for he found in her at last the utterly self-abnegating love that he sought and never found, except in his mother. When she gave herself to him in death she was wholly his, as he could never hope she would be in life. So, to grasp at last the fulfillment of his life-long desire he took his own life, making a consistent closing to the unfolding of his restless spirit stirred always by the "divine discontent." We cannot say that his suicide was that of an insane man, for it is no more insane, in itself, than the acts that many men commit every day in order to gain their most-desired end. Heinrich von Kleist could never live the conventional life because he could never reconcile himself to things as they are. An all-too-early fixation upon an unattainable goal was not broken down, and he pursued the unattainable till he fancied he found it in death.

IX

The Case of Gogol

By Otto Kaus, *Schriften des Vereins freie psychoanalytische Forschung,* No. 2, 1912, 81 p.

In this study the very interesting personality of one of Russia's great literary geniuses is treated with a sympathy and an understanding that no biographer or critic, perhaps, has given it hitherto. The poetic quality of the style in which Kaus has written goes far to increase the enthusiasm of the reader. These very facts of the presentation of the subject make it most difficult to give any adequate reproduction of the work without giving its eighty odd pages in full. In Gogol's life and character the salient points do not stand out so clearly to the layman's view as in Leonardo da Vinci's or Richard Wagner's, and it so happens that in order to make the analysis clear almost all the data and explanation brought up by the author (Kaus) are needed, and this mass of material cannot be presented in five hundred words. I am reduced therefore to giving a mere shadowy outline, a suggestive sketch, of the work in question, admitting that it is incomplete, and unjust to the original.

In Gogol's works there is a contradictory completeness and incompleteness, as if they presented the whole of existence, yet only in fragments, that suggests the incompleteness of Gogol's experience contrasting with his power to reproduce all the life that played itself out before his eyes. He was a realist who found his one satisfaction, his one avenue of self-expression, in the creations of his imagination. Yet though he lived through his fiction he could cast this fiction only in the mould of real life. (He was a true neurotic of the type that has a real antagonism to fanciful or artificial additions to a tale "because the actual happenings are so much more interesting"). Gogol must live in his pictures, with the compensation of realism for their fictitiousness, because he was a failure in every department of active life that he tried. This failure may be due partly to a poor physical and mental inheritance but is undoubtedly largely conditioned also by childhood influences.

Gogol was for many years the only child. His father was an entirely insignificant writer of plays. His mother was apparently a negligible character. At any rate she plays small part in Gogol's inner life, and he never depicted a strong female character. The only child of parents to whom he could feel himself superior, he developed a life-long tendency to stand apart from others, to be different. In school, whither he went at the age of twelve, as in after life, he made himself different by failure, if success were impossible. He could not be a popular leader, therefore he would not be a follower; he made himself disliked. He could not win honors, so he dropped out of the game and played by himself. Here at school his first artistic impulse emerges, bred by his desire to assert himself in a way all his own. He wrote satires, and dramas in which he acted, with school-mates, holding up teachers to ridicule. In his attitude toward teachers and authorities, as in his early desire to draw, and to act plays, he betrays the familiar hostility to his father, whose inferiority to himself he would prove. In him the creative impulse was not a retreat from life, however, it was an attempt to find a bridge between his soul and life. He never ceased trying to touch hands with Life. The feeling of *Minderwertigkeit* never leaves him and he never ceases striving to disprove his inferiority. When his aggressive feelings were strongest he produced most. Failing as an actor he became a poet, recapitulating the experience of his school days. He failed also as a teacher and as a government clerk, but he succeeded gloriously as poet and novelist because in this field he could stand alone.

His paranoid traits, so clear in the madness of his closing years, are evident in many actions and attitudes of his life. He left his home in "Little Russia" and went up to Petrograd as a prophet and a Savior, almost as the boy Jesus went to his Father's house in Jerusalem. He was to save Russia, morally, and to glorify her. In all his relations to his motherland is seen the working of the father-complex, with its ambivalent content. It was long before he could free himself and go abroad, both in fact and in his fiction. His first works are *genre* pictures of Russia, his last, however, are Russian only in name. His last act of defiance was the burning of the second part of his last work, his greatest, "Dead Souls." He felt that in this second part he had failed, and he burned it, winning his victory, like many an over-sensitive soul, by refusing to fight.

In all of Gogol's works we may find his spiritual antagonisms and struggles embodied, from "Hans Küchelgarten," to "Dead Souls." His drama, the "Revisor," seems an epitome of the life of the world, played on the narrow stage of "Little Russia." Gogol's abnormal sensitiveness enables him to project and embody the warring impulses in himself in characters representative of humanity, while his very limitation, his weakness, gives him the power to take up accurately and reproduce faithfully life as it is, with the intense realism of the over-determined dream of a neurotic sufferer.

Lack of space makes it impossible for me to give even a part of the fine psychological analysis of the characters and motives of Gogol's works, and the sure tracing of these creations of imagination back to the events of Gogol's life, both the inner and the outer life.

One such motive that must be mentioned in addition to those already given is the struggle between the two sexual components in Gogol, the masculine and the feminine. Woman played little actual part in his life, from childhood on. He never had a successful love affair. One could almost be certain that his character predestined him to such incompleteness of experience. This absence of real experience had its counterpart in his soul-life, wherein the feminine quality struggled always with the masculine—and neither won a victory. His reaction to this struggle expressed itself in most of his works in the delineation of woman as evil, as repulsive, a malign creature. Even in the love plots the hero fights, as the culmination of his battles, *with* her, not *for* her. He fears her, and expresses his *Minderweitigkeit* in letting her be the conqueror. His failure to conquer woman, either actually or psychically, drove him to the "religious mania" described by his biographers. He became a mystic and sought salvation by fasting and self-denial. He had suffered all his life from a "complex" of the fear of death. Now at last he seeks to conquer both his fears, fear of the "ewige weibliches" and of Death, together, by seeking and defying the latter. He elects to die by fasting and he dies while kneeling before the picture of the Mother of God. "Groaning and crying out with his last strength, he had dragged himself to the symbol of the highest feminine completeness, and when he found the "Glorious Virgin" of his dreams his dissolution came. Did he seek a victory? a last surrender? Probably both together. He had overpowered Death and Woman, in that he gave himself to them."

So his closing scene is typical of the strength and the weakness that made the character and life of Gogol. The contrasting completeness and incompleteness of his work finds its counterpart in the comprehensive struggle of his life, where he met most often defeat, and never full victory. His failure to find the "pure form" which he sought, except in death, made him perhaps, a martyr. His difficulty in bridging the chasm between his solitary, child-like self, and the real world, made him a great creator of fiction, a practical failure, and a madman.

X

RICHARD WAGNER IN THE "FLYING DUTCHMAN"

By MAX GRAF, *Schriften* IX, 1911, 45 p., and

THE LOHENGRIN SAGA

By OTTO RANK, *Schriften* XIII, 1911, 181 p.

Richard Wagner in the "Flying Dutchman." A contribution to the psychology of artistic creation, by Max Graf, 1911.

The life, the character, the work, and the genius of Richard Wagner are so far removed from the ordinary that it is not strange to find the psychologist and clinician yielding to the fascination of the study he offers and each trying to find the spring of the mystery by his own peculiar divining rod. Dr. Max Graf has brought the psychoanalyst's rod—the infantile complex—to the solution, and has given us a most interesting contribution to the understanding of Wagner's character from the starting point of his opera "The Flying Dutchman." This opera, the second of Wagner's works, is noted to be in sharp contrast to his first work, "Rienzi." Moreover, the themes that work themselves out in "The Flying Dutchman" are repeated with variations in all the later operas. The principal one of these is the theme of the good and true woman contended for by two men, to one of whom she yields herself wholly, having, in some of the stories, seen him previously in dreams, and whom she saves from some impending doom by her faithful love. In the "Flying Dutchman" it is Senta, in "Tannhauser," Elizabeth, Brunhilde in the "Twilight of the Gods," Ysolde in "Tristan and Ysolde," and the theme enters partially into "The Master Singers of Nuremberg," comedy as it is, in Eva. One and all of these operas express a high ideal of womanhood, and of manhood as well, though the hero is represented oftener as struggling and falling. In the "Flying Dutchman" is found the theme in its greatest simplicity.

About a year before the first sketches of the Flying Dutchman were completed Wagner had made the voyage in a sailing vessel from Pillau to London, after his flight from Rigà. He had previously read Heine's version of the Flying Dutchman's story but it was during this voyage that the story became impressed upon his mind, and that he obtained the setting and the atmosphere for his opera. The story relates that a Dutch captain is condemned to sail the seas forever, landing only once in every seven years, in order to search for the woman "faithful unto death" who alone has power to save him from his doom. He finds her in Senta, the daughter of a Norwegian merchant. Senta has already fallen in love with his portrait, which, handed down from her ancestors, hangs upon the wall of her home. Absorbed in fancies concerning the portrait, she is deaf to the wooing of her lover Erik and when the handsome original of the picture appears she yields her heart to him. They are betrothed, but before the wedding can take place the Flying Dutchman is impelled to set forth once more upon his endless voyage. He bids farewell to Senta, declaring that he will not involve her in his doom. She is determined to follow and share his fate, so when he embarks without her she throws herself over the cliff. As she sinks the phantom ship of the Dutchman sinks also, and the souls of the two lovers are seen floating upward. The spectators perceive that the Dutchman is released from his curse at last, by the power of the love of the Woman, "faithful unto death."

In this opera we have the expression of more than one trend of Wagner's character. The unrest and necessity to wander embodied in the Dutchman are paralleled by Wagner's own restless, troubled, wandering life. Finck, in his "Wagner and his Works," says "Wagner was born with the instinct of Travel." (Finck, p. 23.) That the phantasy shaped by his creative spirit is the attempt to satisfy his wild and unconquerable longings Wagner himself has testified. He writes to Liszt, in 1852:

"Of the real joy of life I know nothing; for me the enjoyments of life, of love, are only objects of imagination, not of experience. So my heart must enter into my brain and my life be only that of an artist; only as an artist can I live, in that is my whole being contained." (Graf, p. 45.) Again he writes,

"I cannot help knowing that if we had life we would have no need of art . . . art begins where life leaves off; when there is nothing more here, then we cry out, in art, 'I wished.' Is not our art merely a confession of impotence?"

If his own testimony be true then surely we may find in his strange, beautiful, and moving dramas the key to his artistic soul. We do not leave out of consideration the facts of his adult life, that he was oppressed by poverty, harried by illness, that his domestic relations were not happy, and that he was embittered by the persecution of his enemies. But in order to probe the problem of his character to the bottom we must go back of these phenomena to the earlier influences that surrounded him.

A significant point in connection with his childhood is the ambiguity of his paternity. When he was six months old Aktuarius Friedrich Wagner, probably his father, died, leaving his widow with seven children. Ludwig Geyer aided the struggling family and nine months later married Frau Wagner. Richard was enrolled in school as Richard Geyer, and this has contributed to the confusion that has led some biographers to claim that Geyer was the father of Richard. Wagner himself in later life frequently spoke of Geyer as his father, and honored and loved him very deeply. Sometimes also, however, he calls him his Uncle. As he was his stepfather, and the only father he ever knew, none of this is surprising. Nevertheless it is not unlikely that he himself was uncertain of his paternity and that this gave rise to a complex—a complex that is by no means uncommon in children, who often fancy that they do not belong to their seeming parents. Richard loved the kind stepfather and would fain be his child; so, too, like other children, he would be differentiated from his older brothers and sisters. In later life he showed the influence of Ludwig Geyer in many ways, even to his manner of dress.

The other side of the infantile picture is his deep love for his mother. As a sickly child, and the youngest, he met with the overtenderness that is found to lie at the basis of sc many neurotic characters. He came to love his mother precociously, with the natural consequence that he wanted her all to himself, and would like to be everything to her, taking his father's place. The doubt of his paternity complicated this constellation further. Like the stepfather he is a rival, in his subconscious wish, to the real father. He would win his dear one, as Geyer did, in the face of an older rival, he would sweep all before him while the rival remains powerless. This situation is worked out in the triangle of almost all his operas subsequent to Rienzi,—in the Dutchman, Senta, and Erik; in Elizabeth, Tannhauser and Wolfram; in Brunhilde, Siegfried and Gunther; Ysolde, Tristan and Mark. There is much testimony as to Wagner's extraordinary devotion to his "dear little mother," both in his own writings and in that of contemporary biographers. Praeger writes, "I verily believe that Richard Wagner never loved anyone so deeply as his 'liebes Mutterchen'." (Finck, p. 12.) The conflict between mother-love and father-love, unconscious as it is, lends the tragic tone that impels to the creation of mysterious difficulties and dooms besetting and threatening the different dramatic heroes. Wagner identifies himself with Geyer, and later, as he has indirectly testified, with the heroic creations of his fancy, while his mother is the prototype of the gentle, tender, but dauntless Sentas, Elizabeths and Ysoldes, who respond so quickly to the spell of the true lover and who maintain their faith unto death. The recurring theme comes out first in the "Flying Dutchman," as we have noted, which was begun soon after the disillusionment and disappointment of his first marriage had become manifest; it is more strikingly and completely worked out in "Siegfried," composed shortly after his mother's death. (p. 38.) Siegfried embodies his youthful ideal of himself as he would like to be in order to please his mother. All who know the opera will recollect the touching song in which Siegfried tells of his mother

Sieglinde. The description well fits Johanna Wagner, "of the wonderful eyes," as Praeger and others have pointed out. (Finck, p. 12-13.)

In the Meistersinger, furthermore, there is a mother-theme in Walther's story of his dream that almost amounts to documentary evidence that the mother-character underlies all the different shapes which Wagner gave to his noble, self-effacing, faithful, ideal of womanhood. This dream of Walther resembles a dream of Wagner's own, which he relates in a letter to Mathilde Wesendonk, in 1859. He saw two doves flying from her—Mathilde—toward him, while she holds aloft a huge laurel wreath. Then came a storm, with a blinding lightning flash, and he woke. This and other related dreams show with what hopes and wishes his personal life was filled and their resemblance to themes of his operas shows that the latter indeed served the purpose of wish-expression and vicarious fulfillment. It was after the disappointment of his first marriage, and with the effect of other disappointments and hardships also, that he was driven back into boyish dreams again, and in these he sought henceforth, through imagination and artistic expression of his imaginings, the satisfaction and joy of Life and Love that he never found in reality. Back to the shadowy satisfaction of dreams and fancies, away from the hard and bitter realities, back to the tender mother's breast, away from the unsympathetic world, as so many a genius and so many a madman has sought to go.

Is it the intensity of his longing, the depth of his disappointment and grief, that give to his music its wonderful emotional power, to his harmonies their strange, original, untrammeled tone-combinations, to his dramatic characters their lofty idealism, too high, almost, for mortals? Or is there some other condition for his genius? At any rate his own insight has given us the key to *one* spring of his power, at least. "Where life ends, art begins," he writes, and "When there is nothing more here then we cry out, in Art 'I wish.'" The Wish, by the law of Progression and Regression, is some form of the earliest and strongest wishes that stirred the childish soul, and that never die, although they change their shape. The less they receive satisfaction from life the more they tend to keep their original shape, to re-awaken in old, infantile, settings. So the Flying Dutchman fled back to the land in search of the one true, motherly, heart, and when he found her he entered at last into rest. So Richard Wagner strove to find his rest also in the refuge of motherly arms and boyish fantasy.

The Lohengrin Saga, by Otto Rank, 1911

This elaborate and very interesting study of the origins of the Lohengrin, or Swan-knight, myth belongs rather to the realm of the psychoanalytic study of mythology than to the study of genius. There is one portion of it, however, that has a direct bearing upon the character of Richard Wagner and upon his artistic motives. (Chapter VIII. Die Lohengrinsage.) This study confirms the findings of Graf as set forth in the preceding study and also adds one or two points.

The Lohengrin Saga is shown to be the product of primitive fancies and symbolisms, of common infantile conceptions and dreams, as has been shown in much of the psychoanalytic literature, notably Abraham's *Dream and Myth,* Rank's *Myths of the Birth of Heroes,* and Riklin's *Wish-fulfillment and Symbolism in Fairy-tales.* This myth, like many others is a mosaic of race-dreams which makes its perennial appeal because of the answering chords it strikes in individual hearts. Wagner bears testimony to the strong hold that the story took upon his mind and heart and freely admits that the Swan-knight embodies many of his dearest dreams, and that in this creation of imagination he seeks self-expression. (p. 132.) He realizes that this hero, like most of his other characters, is egocentric, pointing back to the poet himself. His choice of the material for his opera was neither accidental nor determined by purely artistic motives; it came from an inner compulsion, because of the corresponding features in his own experience and in the saga. (cf. Jones' "Hamlet.") In a letter he writes to Rockel (p. 133) "The artist says in the characters created by him: So art thou, so feelest and thinkest thou, and so wouldst thou act, if thou, free from the oppressing might of external life-impressions, couldst act according to the choice of thine own wish."

In Lohengrin especially he embodies the tragedy of genius, *his* genius, which seeks to realize itself in fullest humanity. "With his highest thoughts, with his wiser consciousness, wished Lohengrin nothing else than to become and be a complete, whole, warmly-feeling man, not a god—that is, to be an absolute artist." (p. 133.) It is "the longing out of the heights for the deeps," "out of the sunny splendor of stainless purity for the shadows of human love-embraces." "From this height the Woman drew my longing look," (Flying Dutchman, Tannhauser, etc.), the artist has written. When in his human relations the god-like hero finds darkness and shadow he goes back to his god-hood. Wagner wrote this after the unhappy turn of his first marriage. It was regression due to dissatisfaction, a process often illustrated in clinical study. Lohengrin went back to his mysterious abode when Elsa failed him, as Wagner retreats into the land of fancy. Again Wagner has written, significantly (Fünf Vorlsg. pp. 55-56): "All our wishes and so-called tendencies, which in truth bear us over into the future, we seek to build into tangible realities out of the shapes of the past, in order to win for them the form which the immediate Present cannot give them." This insight of his into the source of his creative work leads us directly to the search for the events of his past that were used by him in shaping his present and future soul-life.

Much of this has been given in the preceding summary of Graf's study. Dr. Rank calls attention, in addition, to the part played in the child Richard's mind by the doubt of his mother's truth involved in the doubt concerning his own paternity. Further, the second marriage disturbed his own possession of his mother just as the advent of a new brother would disturb it. In revenge he seeks, in his dramas, to portray women of extraordinary faithfulness, as embodying his ideal, yet significantly varying this in *Elsa* of the Lohengrin story. Further, in revenge he identifies himself with the father. His feelings find expression in his early work "The Wedding" and in almost every other, where the hero takes the heroine away from a former lover. So too we may mention that Wagner's sincerest attachment, apparently, was to Mathilde Wesendonk, the wife of a friend, and that his second marriage was to Cosima Liszt Bülow, the divorced wife of another friend. A variation of this theme is the winning of a daughter from a jealous father by *Siegfried*. Ambivalance toward the father is shown in the contrast character of the good Heinrich I and wicked Telramund.

Wagner changed the well known Lohengrin saga in one important particular—he had no true marriage take place, and therefore no fruit of the marriage between Elsa and Lohengrin. Since Elsa was an embodiment of his mother and Lohengrin of himself he could not bear the incestuous union. So, too, Lohengrin could not give his name (compare the Oedipus saga) for this exposure would parallel an exposure of incest. (p. 143.)

Wagner is the poet of Salvation—Erlösung. The Flying Dutchman, Tannhauser, Tristan, Parsifal, seek salvation, and with the salvation motif is woven the love-motif since salvation is found through a good, chaste, true, stainless woman, the woman who is the sublimation of his old childish naïve conception, in contrast to the new conception coming, not without a shock, during puberty. This contrast is pictured in Elizabeth and Venus, of "Tannhauser." Protection from all the unpleasantness and evil of the adult world is found in the return to the pure and holy Mother of childhood days. The Rettungs-phantasie, or deliverance myths, are birth fancies, expressing the wish to win the mother to wifehood, while the Salvation myths—Erlösungs-phantasie—represent the wish to be made a husband by the mother. It is the contrast of the active and passive. (p. 149.)

In Wagner's relations to his mother and father and to his wives we find, then, the emotional settings that account for his deep interest in the Lohengrin saga and for the principal themes that are embodied in his great dramatic creations.

XI

THE TURNING-POINT IN THE LIFE OF NAPOLEON I

By LUDWIG JEKELS, *Imago,* Vol. III, 4, 1914, p. 313-381

If any scientist could explain that man who has been hailed as "the greatest of the great," of whom Victor Hugo has said, "He possessed all, he was complete; he had in his brain human capacity to the sixth power"—if anyone could explain the supreme genius of *Napoleon,* he would render a service not only to History but to the whole group of sciences—Psychology, Sociology, Eugenics, etc.—that deal with the study of Man. To this problem the psychoanalyst brings his method, and, while he does not succeed in accounting for the overpowering greatness of the man, he has thrown a light upon some of his leading characteristics, upon one of the most significant periods of his life, and upon his over-mastering ambition and egotism. Napoleon's fatherly solicitude for his family and his people stands in sharp contrast to his relentless cruelty, his ruthless and Oriental despotism. His astonishing ambition seems paranoid in its boundless audacity, it would be almost ludicrous were it not tragic, yet he was a man of remarkably sound judgment and good sense both as a general and as a statesman.

Aside from these and other contradictions in character, to be treated later, his life falls into two contrasting chronological halves, in the second of which he holds views and sentiments that are exactly the reverse of those animating him in the first half. It was in or about his twenty-fourth year (1791-92) that the change came which is here called the turning point in his life. It was at this time that he changed from an ardent Corsican patriot, hater of France, and Anglo-maniac, to a patriotic Frenchman, and an enemy of England; from a condemner of Alexander the Great to his ardent admirer; he turned from a limitless reverence for Rousseau, to an attitude of contempt, calling him a weakling and a fool; from a Jacobin, an advocate of equality, and an enemy of kings, to a general who loved to surround himself with full royal significance and state—as in his Italian campaign—and finally to an absolute Emperor, seeking equality, it seemed, with Divinity itself. (p. 340.)

We find, for the solution of the problem raised by these contradictions, much in Napoleon's own writings that will furnish the key to those able to grasp it. For example, in his Corsican period, he clothes his accusations against his father and against traitors of other times in such words as the following: "They have brought strangers against their *Mother-land.* . . . They have agreed to union with a stranger." (p. 340.) The concept of the land as a mother, and the hatred of strangers permeates his thought and action. The concept of Mother Earth is, of course, a very ancient one (see Greek and Roman myths, Livy, I, LXI, Otto Rank "Inzest-Motiv in Dichtung und Saga") and its universality shows to the psychoanalyst that it has a root in infantile psychology. Now if we suppose that Napoleon, like many other youths, suffered from an Oedipus-Complex, we find that his conception of the mother and the stranger, his ardent patriotism, and its sudden transference to another object, his ambition to possess the whole earth, and his assumption of despotic (paternal) power, will, with many phases of his private life, fall into place as natural consequences. It is true that we have no direct evidence for the existence of such a complex, and in assuming it we reason from effects to cause, but the effects do fit such a cause with admirable precision, so the explanation is offered as a more adequate one than any that has been put forth hitherto.

The facts, from this point of view, are as follows: Napoleon loved his mother, as a child, in an infantilely sexual way and this mother-fixation persisted throughout his life. The complex so formed was further complicated by the circumstances of his father's absences from home and his mother's intimacy with the French governor (p. 340), Marbeuf,—the stranger. Corsica, his native land, became identified with his mother in the age-old, natural way, and France was the usurping stranger. The unconscious animus against the father strengthened the animus against the stranger also. Paoli, the great Corsican patriot, who led in the Corsican revolt against Genoa, but supported the union with France, was at first much loved and venerated. He was the ideal father, the protector of the mother. (p. 373.) The King of France was another surrogate-father—as all kings and rulers are "Fathers." The death of Napoleon's father initiated the change in sentiment, partially freeing the complex, liberating his own fatherly trends and turning him against Paoli. The death of Louis XVI of France completed it, logically proving, first that all fathers must be removed (as all sons know)—p. 370—and giving room, second, for his own succession to the paternal position.

Various phases of his private life show the workings of his complex. It is notorious that he loved women older than himself and women who were unfaithful, while he was brutal to the young and the virtuous woman. (p. 355.) Thus he sought the likeness of his mother in other women. As soon as his father died he assumed responsibility for his brothers and his mother, although he was not the eldest, and gave to all a true fatherly care that never failed throughout his life. When a boy at school in Brienne he fenced in a little garden spot, tended it devotedly and showed fierce outbursts of almost insane rage if a comrade dared to trespass upon it. Doubtless this garden spot was his symbol of mother-earth and he would not share his possession of it. His fiercest outburst happened upon the day dedicated to St. Louis and celebrated in honor of King Louis, in which celebration he sullenly refused to participate. This little incident has an obvious significance for the interpretation of Napoleon's life here set forth. As he was freed from the paternal dominations of various sorts his own paternal character expanded, and repeated indications of identification with his father occur. His pleasure in calling himself Charlemagne and in imitating that greatest French monarch may have been further stimulated by the unconscious identification of Charlemagne with *Charles-Marie*, his father. In turning against Paoli he identifies himself with his father, who had done the same, at the same time carrying to completion the gradual liberation of the libido to be expressed by the unconscious cry "Down with Fathers!" In turning against Paoli he must turn also against Paoli's love for England (and he himself had been called an Anglo-maniac). (p. 373.) With the emancipation from paternal and monarchical rule comes the freedom to possess, and France also becomes a mother—and a far greater mother than Corsica. From now on the expansion proceeds, until it takes the whole earth, and perhaps the heavens as well, to satisfy his libido. Italy came after France—his mother was Italian, and he often wrote his name in the Italian form—then it took in Europe, and he had a frequently uttered eagerness to possess Asia and to be an Oriental monarch. (p. 377.) That astounding ambition that causes one half the world to heap his memory with execrations and the other half to surround it with expressions of admiration, took its rise, then, at the time when the extermination of two types of father resolved his conflict and left him free to seek the fulfillment of his wish in all its widest sublimations, in full possession of his mother—Mother Earth. The circumstances of the times were fully favorable to such ambition. The Revolutionary spirit sought to free itself from all traditions and restraints of authority, and to assert its Selfhood. So Napoleon identified himself with the spirit of his time, gave it its fullest expression, and then dominated it, as, after the revolt of young manhood against the restraints of the family, the maturing man seeks to rule, in his turn, a family of his own. Such a process is common, both in the private, sexual life, and in the wider life of work and social activity, which is regarded as a sublimation of the first.

Napoleon differed from other men in the intensity and degree of his complex, and, very probably, in innate dispositions that we cannot yet analyze. The times and circumstances of his life, the political situation, certainly had much to do with the momentousness of the results of his activity and also much to do with the paths that activity took. But he cannot be accounted for fully, according to our present knowledge, either by the world-situation during which he appeared, nor by the psychoanalytic explanation here offered. It is felt however, that the method of psychoanalysis has made a definite contribution to the understanding of the intense power and momentum of the man which carried him so far beyond the middle levels.

XII

The Case of Louis Bonaparte, King of Holland

By Ernest Jones, *Journ. Abnorm. Psychol.*, 1914, p. 289-301

A study of Louis Bonaparte is given here not because he was a great man—for assuredly he was not—but for the sake of the contribution that an investigation of his case can make to the understanding of the character of his marvelous brother. A psychological analysis of Napoleon Bonaparte could not be complete without a study of his family. If Napoleon shows what strength and power, what resistless force, may come from an unsatisfied and neurotic wish, which destroys the equilibrium of the personality, Louis shows how the same neurotic inheritance, working in a different manner, and upon different material, may produce the weakness of a paralyzed will.

Of his younger brother, Louis, Napoleon entertained the highest hopes, even thinking of making him his successor. He made Louis his favorite, personally taking charge of his education, and treating him with more than fatherly tenderness. Louis justified and repaid his brother's care and confidence with obedience and devoted service, up to his twentieth year, then he had an illness which was followed by a change of character and conduct. From this time he failed his brother repeatedly, refusing to obey his commands, acting in direct opposition to his wishes, or showing marked indifference and incompetence when he seemed to comply with Napoleon's orders. He made his ill health serve often as an excuse for his dereliction and indeed he was henceforth a confirmed nervous invalid. The climax came when he was made King of Holland, in 1806. He promptly conceived of himself as representing the individual interest of Holland rather than the large interests of the Empire and of France, and so put every obstacle in the way of Napoleon, finally precipitating the incorporation of Holland into the Empire and thus bringing one more nation into the coalition against Napoleon when the revolt came. After three years Louis abdicated and retired to Gratz, having failed his brother at every crisis. His conduct in the military campaigns in Egypt, Italy, and Russia, was of a piece with this. "During the whole of Napoleon's period of power Louis either refused to co-operate with him or else did so only very grudgingly and half heartedly. Yet there were occasional moments even in this time when his old devotion to his brother reasserted itself, particularly when the latter seemed in danger . . . and again . . . after his downfall." (p. 7.) In later life he busied himself with replies to his brother's detractors and firmly believed that Napoleon was the victim of persecution and that his every failure was due to the machinations of enemies. He never knew that his own conduct had not only wrecked his own career but thwarted the projects of Napoleon and changed the course of history. From the standpoint of the last named fact, an adequate explanation of his conduct becomes of value not merely for psychology but for history as well. The explanation can be found, the author believes, in the erotic relations of Louis to his brother.

That the affection of the brothers for each other was intense in early days there can be no doubt. Napoleon had [illegible] in praise of Louis' good qualities, while Josephine sa[illegible]

"He loves Bonaparte as a lover loves his mistr[illegible] he wrote to him when he left Egypt are so tender that they make tears come to one's eyes." Here is direct evidence of the truth that Louis' conduct leads us to suspect, namely that he had a strong homosexual attachment to his brother. Other evidence of his homosexuality is found in his very unhappy marital relations. Just at the date when Napoleon was arranging the divorce from Josephine Louis sought a separation from his wife. This may be a case of identification with his brother. His family life sheds much light upon the homosexual conflict, exhibiting his unreasoning jealousy, his doubts as to the legitimacy of his sons, and especially his suspicion of Napoleon in this connection.

Louis' relation to his brother is one of ambivalence, like the ambivalence of an adoring but rebellious son toward his father. Napoleon stood *in loco parentis* to Louis, and his attitude was just that of an over-fond but domineering and imperious parent. This aroused first a strong homosexual attachment, as Louis was of the feminine type and was attracted by his brother's masterfulness. But this state of feeling set up a conflict with his self-love that persisted throughout his life, resulting in the formation and permanent establishment of a love-and-hate complex. The venereal disease that attacked him was the immediate cause of the rise of the homosexual conflict. This experience had the effect of turning him away from heterosexuality and of producing a marked misogyny, as it has done in other men who are not strongly heterosexual to begin with, and as has notably been the case with Nietzsche. (p. 11.) His brother's treatment of him appeared to Louis as that of a firm but fond parent appears to an ignorant child, that is, as an alternation of love and hate. The fault did not lie in Napoleon, however, but in his brother's childish attitude. While Napoleon was in power jealousy and resentment caused Louis to withdraw from co-operation with him like a sulky, fearful, child, who both will and won't, and his ill health serves as an excuse—it is a protective mechanism against the truth, like so many hysterical maladies.

His case presents a paranoid syndrome of delusions of jealousy and persecution. As has been found in so many psychopathic cases, delusions of persecution are the expression of disappointed love, and are brought about by means of a double inversion of the underlying content. (p. 14.) The love is replaced by hate, and the emotion is ascribed to, or projected on to, the person toward whom it was originally directed. Recent investigations seem to prove that in paranoid cases the underlying conflict is always a homosexual one, so this is evidence also for the homosexuality of Louis. He never became a true paranoiac, however, but a nervous invalid, letting his physical organism bear the brunt of the struggle.

In his political attitude he duplicates the personal struggle, identifying himself with Holland and his brother with France. He was deluded for years into the belief that the Dutch people loved and honored him as their true sovereign, and longed for his return. Likewise, after his brother's downfall, France became a persecutor of his brother as his brother had once, so he fancied, persecuted him. The complex thus undergoes a transformation, when the brother's power is gone, but it is never resolved. Another reason for his later more favorable attitude toward Napoleon is the painfulness of certain domestic relations. He once believed Napoleon to be the father of one of his wife's children. In later life he could not accept this view and as an unconsciously adopted defence maintained that Napoleon had never been unfaithful to Josephine. His delusions however, were never so much delusions as *preconscious* beliefs, which his judgment was able to hold in check. He kept his reason, in the face of the fierce conflict of love and hate, at the cost of his health and his will to act, like so many neuropathic cases in our hospitals and sanitoriums to-day. Such conflicts exhaust the energy of the sufferer, leaving none for will and action.

XIII

AMENHOTEP IV. ECHNATON. PSYCHOANALYTIC CONTRIBUTION TO THE UNDERSTANDING OF HIS PERSONALITY AND OF THE MONOTHEISTIC ATON-CULT

By KARL ABRAHAM, *Imago* I, 1912, pp. 334-360

This able study of personality has a peculiar interest in that it deals with a character out of ancient times, and, in spite of the remoteness of the civilization of Egypt in the year 1400 B. C., brings the king Amenhotep IV to us as a very real and human person. He is one, moreover, whose ideals more nearly approach the modern spirit than any to be found in ancient Egypt.

The material for the study was gathered from the finds at Tel el Amarna, in 1880, as they have been presented by Breasted, Weigall, Niebuhr, Sethe and Flinders Petrie, all noted Egyptologists. And so complete is the data available that Dr. Abraham has no need to fall back upon speculation. The study is founded upon unquestionable facts.

Amenhotep IV, of the 15th dynasty, called the "Heretic King," was the grandson of the great Amenhotep II who completed the work of establishing the world dominion of Egypt, begun by Thotmes III. His mother and grandmother were Asiatic Princesses, the mother being Queen Teje, a woman whose character exercised a most significant influence upon her son. As is so often the case with the descendants of a strong and active character, the son of the war-like conqueror, Amenhotep II, was a man who could barely hold his father's work together, though he also was of the active type, while the grandson was a man of the contemplative rather than of the active temperament—an idealist and a dreamer—and the later descendants were neurotics and weaklings. That is, the first generation, rather overpowered by the might of the parent, is hindered from full development and becomes a pale copy of the father. He enjoys the luxury and power created by his father's efforts to an extent detrimental to himself. The second generation is still further weakened, shows a tendency to over-refined intellectuality, rebels against the ways of his progenitors, and becomes a contemplative, inactive individual, who gives up real life for dreams. Thus it was with the Amenhoteps. The character of Amenhotep IV is clearly revealed to us in the principal acts of his life and reign to have been such as is fitted by the last description.

Like his father he married an Asiatic princess, and he was the first of the Pharaohs to be strictly monogamous. He loved his wife with a tenderness equalled only by that which he had lavished upon his mother. When his mother died he did not lay her beside his father, but beside the tomb prepared for himself. In very many ways he exhibited his resolution to fashion his life not according to the example of his father but to the soul of his mother. He introduced new forms of art, following nature rather than tradition. He used new hieroglyphic symbols, notably abandoning the hawk-sign for *woman* and spelling the word syllabically. Most significant of all, he abandoned his father's religion, polytheism with *Amon* as the principal god, and propagated the religion of his mother, the worship of *Aton,* an Asiatic deity identified with the Greek Adonis. He identified Aton with the ancient Egyptian God Ra, thus avoiding the stigma of innovation, but he admitted no other gods, declaring for absolute monotheism. Moreover, Aton was a spiritual god, not represented by any anthropomorphic being nor by any living creature, but by the rays of the sun—not the sun itself. He was "father of all things," "from everlasting," universal, omnipotent, unrivalled, benevolent, a lover of peace.

Amenhotep did not content himself with his own change of belief. He vigorously rooted out the Amon-worship, struck out the name of Amon from inscriptions, erased his father's name from many, and changed his own name to Echnaton, "the chosen of Aton." Still further, he removed from his father's capital of Thebes, building a new capital called Achet-Aton, near the site of the old capital of the ancient kings—Memphis—to whom he now turned as to his true ancestors. They are substitutes for his immediate progenitors, of whom his spirit disapproved.

Later, he claimed *Aton* as his true father, not in the earthly, but in the spiritual sense; still, in a spirit that shows his secret wish to desclaim his real paternity. Boys of common rank fancy themselves the sons of kings, Echnaton, being the son of the greatest king on Earth, can assert his superiority to his father only by being the son of a god. Such fantasies (if fantasies they may be called) are generally found with a neurotic disposition. Evidence is not lacking that Echnaton showed neurotic traits. The history speaks of visions or trance-states, suggesting either hysteria or hystero-epilepsy. The face as we see it in the bust preserved shows a sensitiveness and over-refinement that often predisposes to the recoil from reality constituting a neurosis.

Turning quite away from the customs of his fathers this idealistic, dreaming, cloud-scaling, young king devoted all his thought and energy to establishing his monotheistic religion and to making moral reforms, with no care for the splendors of world-rule handed down to him by his predecessors. And not only did he bring forth a monotheism and a morality equalling that ascribed to Moses, but also he approached far more nearly to the Christian ideal of deity and of ethics than any pre-Christian teacher. Echnaton fashioned his Father-god somewhat in his own image,—a god of peace, kindness, and good-will. Here once more he shows his antagonism to his warrior fathers. No image was permitted to be made of his God, who, as a spiritual being was thus removed from all rivalry, made a universal, and not a national, god, thus asserting Echnaton's ambitions in a way entirely different from the ambition of world-rule that animated the former Amenhoteps. It is a case of transference and sublimation the most complete.

With all the zeal of the true reformer the young king, who reigned from his tenth to his twenty-eighth year, devoted himself to the work of extending the kingdom of Heaven—and lost his own. Too absorbed in his religious dreams, and in his tender family life—which also shows clearly the results of mother-fixation—to care for the safeguarding of his Asiatic possessions, he let one province after another slip from his grasp. Overrun by hostile tribes his distant subjects appealed to him in vain for help. One by one they were snatched away or they revolted, and the great Empire that his grandfather had labored to build and that his father had zealously guarded, crumbled into ruin. Neither did his own work stand. His people had never really adopted monotheism—indeed they were not ready to ascend to his heights of spiritual vision. After his death they hastened to go back to the old ways. The powerful priests of Amon restored the worship of their deity, Echnaton's work was destroyed, his inscriptions erased—in short, a strong counter-reformation set in.

Down the long vista of thirty-four centuries, Amenhotep IV, or Echnaton, as he wished to be called, stands forth an appealing figure. Physical and mental constitution led to repression of his true self in the lifetime of his active tyrannical father, during the first ten years of life, and to a clinging, mentally and spiritually, to his idealistic mother, the foreign princess. When the time of his freedom came he could not assert himself in the action and turmoil of battle and conquest. He still dreamed dreams and saw visions for which that early and practical age had even less use than later ages. Yet so lofty were the visions and so powerful the personality of the dreamer that he stands as one of the world's religious geniuses, though he could never be what his time and place required—a ruling genius.

XIV

THE PIETY OF COUNT LUDWIG VON ZINZENDORF

By OSKAR PFISTER, 1910, *Schriften* VIII, 122 p.

Ludwig von Zinzendorf, leader of the Moravians in the eighteenth century, and by some German historians ranked next to Luther as a leader, furnishes an example of a pathologically determined genius working in the realm of religion rather than of art, or of politics. He was the son of a strictly pietistic family and his upbringing was such that he could not have escaped pietism—unless his infantile experience had been of the sort that predisposes to revolt, and his was not. Neither could he develop into a normal personality under the conditions imposed upon his childhood.

His father died when Ludwig was but six weeks old. His death-day was observed yearly afterward and this made a strong impression upon the child, causing him to associate Death with Father in a way very significant for his later mental development, as we shall see. The teachings of his mother led him to bring into this complex also a strong interest in the crucified Savior. His infantile piety was guided to worship of Jesus. At the same time his stern mother showed him little tenderness and early sent him from her side, to school. At his pietistic school he was subjected to the severest discipline "for his soul's good," and was maltreated by fellow pupils as well as by teachers. Separated from father, mother, brother and sisters, he could not make the natural sublimations of his libido upon them. He found no substitutes for them and his whole store of love and longing was poured out upon the one whom he was taught most to honor—Jesus. Even in childhood we find him having ecstacies over the contemplation of the bleeding Savior. (p. 8.) And even thus early his feeling toward the Savior clearly shows its fleshly, or sexual, nature. How could it be otherwise when he was denied all natural outlets and forced to "give his heart wholly to Jesus?" He carried out this command far more literally than most Christians do—or should, of course. The expression of his sexual desire became fetichistic, fixing upon the wounds and blood of the Savior. This was on the one hand allied with a necrophilism arising from the aforementioned complex of associations with Death, Father, and Jesus, and was on the other hand determined by the asceticism and repression of his training, in that the sexual complex, being imperfectly sublimated, sought objects as like to the original objects of sexual passion as might be. His theology, his rituals, his catechism, his hymns, show this with a clearness that cannot be gainsaid. It scarcely needs a psychoanalyst to point it out. Through contemplation of the wounds of the Savior he experienced love-ecstasies, even orgasms. The hollow made by the spear wound in the side became the centre of a mass of symbolistic interpretation, and the central point also of the creed and theology of Zinzendorf and his followers. It was the Cleft in the Rock, in which believers were hidden, it was the womb from which believers were born as Eve was taken from the side of Adam. This is the imagery of infantile sexuality. (p. 25.)

Carrying the religious expression of his sexual impulse still further he calls Jesus the Bridegroom and himself the Bride. Jesus is thus bisexualized, being at once mother and husband. In the period of his life called the Eruption period, from his forty-first to his forty-ninth year, the sexual fixation upon Jesus was greatly intensified, and the religious rites prescribed for his followers became religious orgies. During this period he addresses Jesus as a woman would her lover. He calls himself the Rib of Jesus, born from the wound in the side. He gives to the Savior's wounds the credit for the new birth of the Christian, and he henceforth looks upon Jesus as the creator of the world. God the Father is wholly subordinated and becomes a sort of Grandfather—Zinzendorf's own words—while Jesus is the Father. The Holy Spirit is now conceived as the Mother of the Trinity. Everything in daily life becomes a symbol of Christ's blood and wounds. The earthly marriage is but a symbol of mystic marriage with Jesus and should be used as such. Earthly love must be sublimated into spiritual love to Jesus. Zinzendorf's young son wrote at this time, and it is corroborated by others, that "we hear nothing but wounds and *wounds*, and *wounds*, and *wounds* and *wounds*, every hour." (p. 32, 33.) It is at this time that the infantilely sexual character of Zinzendorf's religious thought ran to the extravagant use of diminutives that brought ridicule upon the Moravian Society. He talked of the "Little Side Hollow," of "woundlets," of the "Birdlings of the air of the cross," of the "wound-beelets"—believers who feed on Jesus' blood as bees upon honey—and even of little wound-fishes—believers swimming in His blood—a whole menagerie. The dead body of Jesus is exalted, its lifelessness satisfying the requirement for purity. He has intense sexual desire for the corpse of Jesus (necrophilism). He composes a Wound litany which is a religious orgy, sickening to the sane reader. He differs from others of the religious Blood Cult, in that for him the sacred blood is not for cleansing, but to be *fed* upon. So the Lord's Supper, the love-feast, the foot washing, all prominent institutions of his followers, show their sexual import very clearly. The love of Jesus with Zinzendorf had nothing to do with the ethical character of Jesus. No, the Savior was the object of a grossly sensual, erotic desire. The sensual side of the sexual object completely overwhelmed the ethical being. The love of Jesus has always been used by Christians as a prophylactic for sexual love, but with Zinzendorf it is not prophylactic, not sublimated—it *is* sexual love, and homosexual love.

Zinzendorf's relations with his wife were cold and formal. During the period from 1741 to 1749 he met Anna Nitschmann whom he afterward married. There is no doubt that he loved her but he concealed his love even from himself, by making it a symbol of his love for the Savior. She shared in his obsession for the Wounds, which his first wife, judging from her writings, never did. His relations with her during this period probably contributed to the intensifying of the sexual fixation upon the wounded Savior, since his attraction toward her necessitated a very strong repression of natural instinct. He was unable to get away from sexual expression of a somewhat literal sort and so he sexualized religion until his version of Christianity bore very small resemblance to that originally taught by the Bible.

After the Eruption period there is some toning down of the excesses of the Society. The brotherhood was falling into disrepute, and upon representations by one of the brethren—von Peistel—being made to him, Zinzendorf forbade the use of diminutives with sacred words, suspended the love feasts, and gave strict orders about the association of the sexes in religious ceremonies. His inner attitude is not changed, however, although he expresses it less ardently and extravagantly. He still rhapsodizes over wounds and the hollow in the side, though he uses no diminutives. Meanwhile (1756) his wife died and he married Anna Nitschmann. This permitted him a more direct satisfaction and tended to modify the religious extravagance, although the libidinous trend was too firmly fixed to be completely transformed. The infantile determinants remained unchanged. It was probably an infantile fantasy about the deriving of woman from a rib of Adam —a fantasy common in children—that shaped much of his religious thought about the One whom he had chosen for the object of all his love. The childish association of Death with father and with Jesus was another influence, as we have seen. These infantile complexes, never given a fair opportunity to develop into natural sublimations, and the boy being driven, beaten, back upon himself all through his harsh childhood, with only the thought of the Savior for comfort, led almost inevitably to the form of religious fanaticism that we have seen. The intensity and singleness of purpose, the fanatical devotion, that arise from the same circumstances, make him a leader of others. He scarcely ranks as a genius, perhaps, yet he was most significant for the religious life of his time.

In removing Zinzendorf from the pedestal on which some have placed him let no one think that the author would belittle religion. Zinzendorf took the very lowest components that enter into religious life and made them supreme, conforming religious doctrine and practice to his own polymorphous perversities, satisfying his ambivalent sexuality, his sadism and masochism. By such an analysis as this we need not fear that Christianity will suffer. Religion that is founded upon Truth can only gain by separation of the gold from the dross.

XV

THE HYSTERIA AND MYSTICISM OF MARGARETA EBNER

By OSKAR PFISTER, *Zentralblatt fur Psychoanalyse,* Vol. I, 1911, pp. 468-486

Margareta Ebner, a mystic and nun of the fourteenth century (1291-1351) who has left us her autobiography, was a clear case of hysteria combined with religious mysticism. Her physical symptoms of hysteria included lameness, partial paralysis, inability to rise, inability to lower the head, headaches, toothaches, contraction of the jaw, pains under the heart causing spasms in which it took three nuns to hold her, alternating chills and heats, feelings as of having the head pierced through and broken, of having the limbs broken and twisted in death agonies, and sharp pains in the heart, the hands, and the feet. All of these were, according to her own account, simultaneous with or closely following religious exercises and experiences of a definite character. Mental symptoms were periodical inability to speak, causing sometimes pain and sometimes joy, fits of laughing and weeping, especially in the choir, hallucinations of taste—sweet—and of hearing, distaste for meat, and, after 1334, cessation of desire for any food, hallucinations, or dream-experiences, of being embraced by God and kissed by the Christ child, dreams and visions of Heaven, of the body of the Lord, of the Christ child, hallucinatory conversations with the Child Jesus, and amnesia at times for her Pater Noster.

She possessed a crucifix that she often pressed to her heart with all her strength, "almost dying with the sweetness of it." She wore on her bosom a small open book with the picture of the Crucified One just over her heart and slept upon it at night. She often stole (!) a large crucifix from the Choir and laid herself upon it, with her heart upon the heart of the carven figure, "And there I lay, pressed upon it," she writes in her autobiography, "till I slept in great peace." Once she wanted to get at a still larger crucifix in the Choir but it hung too high for her. Then in sleep, she saw herself stand before the figure, saw the Savior climb down and allow her to kiss his open heart and drink his blood "whereby she experienced the most intense sweetness." She held long conversations with the Christ child, wherein she inquired if it were true that his mother suffered no pain in bearing him, and like questions. She developed a fetichism for certain parts of the Savior's body, notably the heart and the breast. She experienced the greatest sweetness and joy in repeating His name; thus the name became a surrogate for Himself, to whom her piety was an erotic relation, on one side conjugal, on the other side motherly. With these ecstacies she also experienced great bodily pain, and always suffered severe pains in reading the story of the passion. She looked upon Jesus as her spouse—as nuns must, but she did so in a more realistic sense—and felt all the joys of union. As a reward for her piety she had the assurance—by hallucinatory voices—that Jesus looked upon her as his true spouse, his well-beloved, and his dwelling-place. Her death wish, shared by most mystics, is bound up with her desire to live in true inward union with her heavenly beloved.

Her motherly feeling is brought out in much of her writings about her communion with the infant Jesus, whom she treats as her own child. She prays "her child to wake her from sleep and let her nurse him." She is much occupied with the Circumcision of Jesus and declares "for the sweet circumcision have I great joy." In all her extravagances and her sufferings, however, Margareta was not so extreme as many of the devotees of her time. She practiced no active self-torture, though she suffered the extreme of passively inflicted self-torture. She lived with the utmost austerity and asceticism.

For a psychoanalysis of her life we lack any knowledge of infantile sexual fantasies, such as we expect to find in hysteric patients, we lack knowledge of constant childhood repression and of acute traumata. Nevertheless, the close relation between her hysteria and piety stands out clearly.

Her astasia-abasia always comes at prayer time and is closely analogous to experiences accompanying social life. Her headache and toothache were experienced when reading the Passion story and are clearly connected with the Crown of Thorns and the smiting of Jesus upon the mouth. Her inability at times to lower her head connects with the death of Jesus when he "bowed his head and gave up his Spirit." The pain in the heart likewise reproduces the spear thrust. The occasional aphasia was connected with the dumbness of Zacharias and with Christ's cry, "My God, why hast Thou forsaken me?" The sensation of floating in the air is an identification with Christ in His ascension, and connects with dreams when there is an upstreaming of sexual feeling. The heavy pains under the heart, causing such spasms that she must be held are due to fancies of motherhood, and birth pains. She was often unable to speak from vespers in the mid-week till daybreak on Sunday. This apparently connects with the interval in which Christ lay in the tomb, though why it begins on Wednesday instead of on Friday is not clear. A complete sympathetic identification with her beloved heavenly bridegroom is clearly evidenced however, by the symptoms given, and others that there is not space to mention. The frequent tastes in the mouth are infantile expressions of sexual pleasure. The anguish almost invariably following is masochistic.

Her desire for the crucifix is analogous to the desires of Zinzendorf treated in the foregoing study. Like Zinzendorf she betrays homosexual traits, for she attributes womanly characters to Jesus.

From this study and that made of Zinzendorf we reach the conclusion that the religious erotic does not really sublimate, but simply abates sexual desires. With them the battle of sexual desire is fought in the air. "In place of a transformation of the libido into ethical, productive, social, and cultural activities," writes Pfister, "a mere *elevation* is brought about, for which the honorable name of sublimation is much too good." The object of the hysterical symptoms and of the piety alike is to give the maximal intensity of emotion. There is a curious emotional *polarization* shown in alternating sadism and masochism, in misery and "sweetness," silence and compulsion to speak, weeping, and laughing, etc., that serves this purpose of intensification, in that the impulse being held back, and kept in check by its opposite, for a time, bursts through with abnormal force and gives the high tide of emotion that is needed to gratify the hysteric—who desires to experience pain as much as pleasure. The symptoms of illness are a part of this mechanism, designed to gratify the egotistical craving for feeling. Another cause for the physical suffering of Margareta was, as we have seen, her perfect sympathetic identification with her Heavenly Lord.

Margareta, Zinzendorf, and countless others, exemplify the truth that the Libido can pass over into Religion without transformation, and when it does so, the polymorphous sex components break forth in wild anarchy. Thus abused Nature grimly revenges herself.

XVI

IGNATIUS LOYOLA, FROM EROTIC TO SAINT

By GEORG LOMER, Leipsic, 1913, 187 pp.

This monograph is a historical pathographical study of the founder of the Society of Jesus. The historical material occupies more space than the psychoanalysis, and the latter is not a *psychoanalysis* in the strict Freudian sense. It is not published under the auspices of any psychoanalytic organization, but it offers some material nevertheless, that should be considered in a survey of the literature of psychoanalysis on great men.

Lomer shows that Loyola was, first of all, the product of his race, country and age. "Spaniard and nobleman through and through, Soldier to the kernel of his being," he writes in concluding the study, "full of burning ambition . . . half an idealistic, active, northerner, half an emotional, authority-accepting, southerner." In the second place it was the accident of the wound that crippled him for life that turned the energy normally spent upon war and sex-conquest into religious channels. The force of these energies, suddenly dammed up and diverted into a partially satisfactory new channel, brought about the hysterical disposition that is clearly enough indicated in Loyola's mysticism, asceticism, visions and hallucinations, and the outbreaks of tearful emotion when in religious ecstacies. These ecstasies were surrogates for sexual satisfaction, as in so many religious mystics and fanatics.

It is this combination of overwhelming hysterical emotionalism with the clear cold intellect that made him an organizing genius, that explains the power of Ignatius Loyola and his Society. His exercises, which embody his spirit and genius, play wholly upon the emotions and through them seize and bind the will and the intellect. Loyola had, himself, the hysteric's power to direct and use his emotions to an end, and this explains his genius and his achievement. He was not a religious paranoiac and not a congenital hysteric; his hysteria was acquired in young manhood, and hence he ruled the hysterical manifestations, making them appear and disappear at will. (This does not imply simulation.) Moreover, he was able to impart to others this same power to call forth and use emotion, through his "Exercises." With his thoroughly Spanish character, which bears the masculine traits of bellicosity, fanaticism, intolerance, love of liberty, or, rather, impatience of restraint from without, and the one feminine trait of mysticism, perverted by the onset of hysteria, a personality was formed that could build and set in motion the greatest religious and political machine of history. Had it not been for the accident that deprived him of physical strength he would never, thinks Lomer, have turned to religion as a field for his ambition. His energy might then have been diverted into more numerous channels, of war, pleasure, and normal life, and have accomplished nothing notable anywhere. This point of the analysis is in accord with the view of the psychoanalysts that the achievements of genius are due to a limitation or frustration of natural activity and that the thwarted energy turns to creative work of some sort, in its irrepressible desire for self-expression and imposition of its own power upon the environing world. Such repression and consequent upsurging of energy has always a powerful emotional accompaniment, to say the least, whatever the relations of emotion and energy really are.

XVII

SCHOPENHAUER. ATTEMPT AT PSYCHOANALYSIS OF THE PHILOSOPHER

By EDWARD HITSCHMANN, *Imago* II., 1913, 100-174

Under this modest title Dr. Hitschmann has given a convincing and complete explanation of the character and philosophy of Schopenhauer, along psychoanalytical lines. I am inclined to think it unsurpassed in excellence by any of the psychoanalyses of great men yet presented. In the following pages I shall try to give the leading points of Hitschmann's analysis, without comment.

Every child, says Hitschmann, is, at a certain age, a philosopher; that is. he seeks to learn, first, the origin of himself and of his brothers and sisters, then, later, the origin of all life. This trait, like other infantile traits, may, under conditions favoring abnormal development, persist and intensify in later life, profoundly influencing the personality, developing, as it does, into the speculative temperament. An examination of all philosophical systems would show that they are traceable in part to the temperament of their founders. Schopenhauer forms a peculiarly brilliant example illustrative of this truth as the threads of connection in his case are clearly visible even to the untrained eye.

Heredity and home-environment both predisposed our philosopher to an unusual line of development, to nervous instability. Father and grandfather were extremely active and capable men but high tempered, harsh, and overbearing. The father is supposed to have committed suicide. The mother married for advantage, after an unfortunate love affair, and seems to have been unemotional, cold, and selfish. Both parents were inclined to nomadism. The boy's relations with neither parent were happy. He and his mother could not live together. She cared more for the society of her friends than for her son, was disgusted with his morose ways and unwilling to yield to his wishes.

He held her responsible for his father's suicide and resented her intimate friendships with other men. He grew to hate her, and hence to hate all woman kind; and this hate was stronger, as always, because it followed a repressed love, over-strong, because of his over-strong emotional and sexual nature. The complex formed by love for the mother with its ambivalent turning to hate, was stimulated and developed by the sternness of his father, of which he has written with bitterness, in later life. From these two elements of his infantile complex—the Oedipus complex—we may trace his misanthropy, his belief that the world was made by a devil—the feared and hated father—rather than by a God, and his conception of Will—again the father—as the dominant force of the Universe. From this protean complex, again, developed the fears that tormented his whole life, numerous various, exaggerated fears, of disease, of infection, of thieves, of prosecution at law, of death, of being buried alive when only apparently dead—fears that led him to constant watchfulness and precaution, and had much to do with his pessimistic outlook. His dreams dealt much with death and fear of death. He even has the fear of the unknown, saying (Natur Philosophie 658) that when he has nothing to fear then he fears the unknown horror that yet remains hidden from him but surely lies in wait. All these fears developed from, or are residues of, *pavor nocturnus* in childhood, and these night fears of children are, as the psychoanalytical school of medicine has shown, very closely related to the child's sexual life. The sense of guilt, death-wishes and love-wishes toward members of his family, became converted into fears for himself by a mechanism of the "guilty conscience" seen often enough in the adult, only here it is a guilty *sub-conscious.*

Schopenhauer's own writings give us a fairly complete picture of his emotional life from childhood up, so that we do not here rest anything upon speculation. He speaks of his intense sexual feelings in childhood and youth, and rejoices when, in maturer years, the conflicts of passion are finally stilled. His treatment of the sex theme in his philosophy reveals the importance it had for him, both by the extent of the treatment he gives it, and more, by his contempt for the female and his homosexual admiration for the beauty of his own sex, his passivity in social relations, his seclusiveness and self-centered pride, all of which kept him from friendships of any sort until, in old age, softened by success and popularity, he grew more genial. The withdrawal of the sensitive child, wounded in his self-esteem by the sternness of the one parent and the lack of tenderness of the other, persisted into a withdrawal from the whole unappreciative social world.

Having endeavored briefly to indicate, rather than fully to describe, the process by which Schopenhauer's temperament grew out of his infantile emotional life, we go on to the principal points of philosophy and trace their nature to the same source.

Schopenhauer's one undying service to philosophy is his discovery of Will as a thing-in-itself. Will, according to his teaching, is the one, metaphysical, imperishable force in humanity, a blind, unconscious force, a force not in humanity alone but in the external world as well, a force described best as the *Will to Live.* This will is most characteristically expressed in the sex-instinct, at the philosopher's neglect of which Schopenhauer marvels. "It is not, like other wishes, a matter of taste . . . for it is the wish of which the very essence of humanity is made." "It is a motive so strong as to be always certain of Victory." (The World as Will and Idea p. 602.) "Sexual union is the true goal of all man's desires." The shame and secrecy of the act are accounted for by the very fact that it expresses the will to live—and life is a painful, a horrible, an evil, thing. To the impression of a powerful, domineering, tyrannical parent who imposed his will upon the helpless child we trace the concept of the blind, unconscious, will. To the precocious sexuality of the child, with its consequent conflict and repression we ascribe his conception of the place of the reproductive instinct and his attitude toward it as shameful and unworthy, also, in part, his pessimistic, resentful, and scornful, attitude toward Life as it is. Scorn of his mother—source of his own life—issued in scorn of all life and of the source of all life. From this same source came his conception of the highest good as pure reason, will-less knowledge, for this means a dream of himself as freed from the dominion of both hateful parents. It was just after his father's death that he began to assert his own will and to write. (137.)

In dealing with Schopenhauer's Ethics we approach another side of the complex. His actual disposition, aggressive, angry, giving way to violent expressions of scorn and hatred of his enemies, is in strange and significant contrast to his doctrine of sympathy and his exaltation of the ascetic ideal. Another seeming contradiction is his sympathy for animals, in view of his implacability toward his fellow men. But the ascetic ideal is merely the conscious expression of his subconscious feeling of guilt, issuing in a masochistic desire for the admixture of pleasure and pain found in doing penance, in self-denial and renunciation. As for the affection and sympathy shown toward animals, that is rather a commonplace compensation used by the hater of humanity as one solvent for his conflict.

Schopenhauer relates that in his seventeenth year he was strongly seized with that same misery over life that Buddha experienced in his youth. It seems significant that this feeling arose in its strength just after the death of Schopenhauer's father. It is an attempt at penance for the wrong he has mentally done to his father in his old, infantile attitude of jealousy and hatred, a compensation for the pleasure he secretly felt at the fulfillment of his wish to have his father out of the way.

We have already touched upon the source of his pessimism, but this point requires some further elaboration. This pessimistic philosophy was but the reflection of his own pain and woe. Much of his misery arose from his inability to adapt himself to the society in which he found himself. His revenge is taken in belittling and hating those from whom he failed to win recognition. The mechanism of resentment, from which Schopenhauer suffered, consists in this, that through disillusion, defeat, non-recognition, with their consequent feeling of inferiority, a sort of mental self-poisoning takes place, while the humiliated and disillusioned man represses into his unconscious soul his revengeful feelings and impulses, his hate, anger, and scorn, instead of subduing them. Life does not yield what was expected and he begins to feel himself lacking in ability, he suffers from a dark conviction of his own incompleteness. All those objects, powers, and virtues which are unattainable he begins to depreciate and undervalue. All this leads to a falsifying of the world-concept; not merely a conscious falsifying, but an unconscious as well, for the new, false, scale of values is carried over into the unconscious, beyond the reach of Reason. It was this mechanism that was responsible for Schopenhauer's philosophy of pessimism, the mechanism itself having been set off originally by the complex arising from his family constellation. The fleetingness and unreality of all things, as taught in his philosophy, borrowed from the Vedantists, is his compensation for the unbearableness of life as it is.

In earliest infancy he had met an intolerable situation. He must share his mother first with his father, then with a little sister who arrived while he, in his ninth year, was among strangers. Feelings of neglect, suspicion, jealousy, grew into actual hatred and scorn of his mother, and this emotional attitude was reinforced by the fantasies incident to puberty and by his mother's actual conduct. The complex extended to include all women, all rivals, all which crossed his will, all which failed to satisfy his ego. He has been compared to Hamlet and has compared himself to Oedipus. This is suggestive of an unconscious emotional bond among these three. Schopenhauer also revenged himself upon his father by a surrogate, through a rebellion against [illegible] and religious authority, and against his Fatherland and [illegible] For all good and pleasant things, moreover, he became [illegible] color blind, because of the poison generated in his own system by disillusion and guilt. Driven back upon himself he becomes self-engrossed, and the real world is for him, in truth, his own consciousness.

We have tried to show that every feature of the personality of a man, the characteristics of his individuality, his capacities, his vocation, his eccentricities are to be explained—if we leave out heredity and the effects of experience—by his instinctive orientation and his lot in very early days of childhood. That is to say, by the family constellation. We have also tried to show that the ground work of a philosophy is derived from the unconscious soul of its creator, and to uncover the mechanism by which a particular view of the cosmos is constructed from known unconscious components. Much of the knowledge necessary for such an attempt at analysis has been derived from the study of neurotic patients, for the components that enter into the neurotic make-up are almost precisely those that enter into the genius. The genius, however, has the gifts of energy and concentration enabling him to find self-fulfillment in work toward a definite end. Schopenhauer, as a child, had the instinct to question the Why and How of things, as all children have. This questioning tendency has a sexual origin. The sexual element becoming repressed, it finds an outlet, with him, in intellectual investigation. In a neurotic of feeble constitution it might have resulted in helpless doubt and inaction. In a normal development it is sublimated, with other childish instincts, in its proper place and relation to the development of the personality as a whole. If, as in Schopenhauer's case, there was an unusually strong sexual (sensual) tendency, the repression leads to a reactive preoccupation with the super-sensual, with Death and the Beyond. The energy of the sex-impulse turns to philosophical thought and reasoning. The philosophical systems of all philosophers are but the projections of their own personalities.

That Schopenhauer himself understood to a great degree the rôle played by the Unconscious is shown in many passages of his works. A few of those cited by Hitschmann are: On dreams, "Natur Philosophie" 145, 146; On sex, the whole "Metaphysik der Geschlechtsleben;" On the unconscious nature of creative thought, "Natur Philosophie," 630, 652.

The "Will" of Schopenhauer is the "Unconscious" of the Freudians, the "Folk Soul" of G. Stanley Hall, under another name.

XVIII

SOME OTHER STUDIES

Among the psychoanalytic writings of the last four years are found several fragments of analysis of genius, that is, analytic treatments of some phases of the character or the work of talented men, that are yet not complete enough to be classed as explanations of the artistic or executive qualities of their subjects.

Such are the studies of the childhood of Tolstoi and Fouqué, under the heading "Of the True Nature of the Child Mind" in the *Imago*, by Hug Hellmuth, Lorenz, Reik and others, an article on "Socrates in the Light of Modern Psychopathology," by Karpas, in the *Journal of Abnormal Psychology*, September, 1915, and more extensive studies of two German dramatists, Theodore Hebbel and Arthur Schnitzler, two studies of the former by Sadger, one of the latter by Hans Sachs, one by Reik, and one by Carl Fortmüller on Schnitzler's *Tragikomödie*, "Das Weite Land." This work may be worth summarizing briefly, as it is suggestive of work that might be done along this line.

Tolstoi's childhood is rich in material and his after life, his ideas, and work, are a field in which the psychoanalyst should surely be welcome. Family constellations abound in his childhood. Both parents died early. Every memory of his mother's voice and manner in later years made him thrill and palpitate. Because his parents did not sympathize with his extreme emotionality he thought them indifferent, and finally conceived the notion that he was a foundling whom they had taken for pity. It became a rapture to think himself miserable, made so not by guilt but by fate.

His reverence for his father was so great that he unconsciously resisted it throughout his life. His father was conceived as belonging to a world of higher beings, whose life was full of secrets. Doubt of his father's goodness led by unconscious paths to doubt of God, and longings for death. In the death fantasies resentment against his tutor, whom he found too severe, played a leading part. The fantasy culminated in dreams of floating away to heaven with his mother. At the age of eight he was obsessed with the idea of flying—as a result of the father-mother fantasies—tried it, came to grief, and thought of it no more. All this is suggestive for thinkers of the psychoanalytic school and fits in well with their theories. Ossip-Lourie, in his "Psychology of the Russian Novelists" takes a different view. He holds that Tolstoi's doubt was the perfectly normal reaction of an intelligent man to the absurdities of religion, and that it was his later "conversion" that was pathological, that the key to his mature life and thought is senile and religious mania, due to a weak constitution.

Hans Freimark, writing upon "Tolstoi as a Character," making, as his sub-title announces, a study on the ground of Tolstoi's writings (*Grenzfragen des Nerven- und Seelenlebens*, 1909), gives another view of Russia's philosopher. According to his view the main-spring of Tolstoi's character, paradoxical as it may seem, is Egoism. Because of his overpowering egoism he was irresistibly drawn to the simple life and unalterably opposed to Culture and the benefits of wealth. Because of this he felt that overwhelming sense of guilt that drove him back to religion and made humility and non-resistence the core of his religion. It was this that made him misjudge and condemn the church. It was to save his own son that he lived, ostensibly, for others. From his egoism came his over-mastering impulsion to be a teacher. This essential element was the same in his childhood, his youth, his old age. The changes in his life, apparently in his thought, were merely external. In new forms he expressed always the same spirit, there was no true "Conversion."

It was his supreme need of self-expression, of self assertion, of proving himself independent of all that was given him, that was forced upon him, that drove him back to the simple, back to poverty, back to the life of the ignorant peasant. He had everything that can be conferred upon man by fortune, wealth, education, friends, liberty. These left nothing for him to achieve, or so it seemed, so he swung to the other extreme, flung off the hateful burden, denied the worth of the gifts of fortune, and strove to make himself heard [illegible] the world without the aid of anything external. From [illegible] springs that spiritual pride that leads him to praise [illegible] to exalt the principle of living for others, as [illegible] opposites of the principles to which his birth and advantages naturally disposed him.

In addition, Tolstoi was a materialist, and what was not material was incomprehensible to him. It was this materialism, as well as his egoistic independence, that made him turn to peasant-life, and away from the church. His worship of simplicity is greatly augmented by this materialistic trend of his mind. The mystic side of Christianity could not exist for him. He could understand immortality only as an extension of the personality by means of love to the neighbor, so that all existed together, all in one, and each in all. But he never makes himself clear on these subjects because he never had the courage to face the truth as it really presented itself to him, just as he once lacked the courage to commit suicide when that was the only logical outcome of his interpretation of life. He took refuge in the assertion that he felt the duty of living in order to serve others. The same weakness is shown in his compromise in giving up his property, for he gave it not to the poor but to his wife—and in his wavering attitude about sex, condemning all as impure and yet again exalting productivity. Such inconsistencies came from his extreme subjectivity. In his egoism he could never see the viewpoint of others, his world was but a projection of himself. He conceived the doctrine of non-resistance, because this expressed his own reaction to culture. Non-resistance is the opposite of all the commands that Culture (*Kultur*) lays upon us. He rebelled against all that Culture represents in order to assert his own individuality. and so his reaction carries him to the opposite pole of progress—to non-resistance. It is one more asseveration of the doctrine he adopted, "He that loseth his life, saveth his life." He laid down all in order to preserve his own ego, in order not to be submerged by things larger than himself.

Because he could never, safe-guarded by Fortune, know any external need, he had to create an internal need for himself, to set up an unattainable goal and to make compromises with life in order to reach it. Only in meeting need, in overcoming difficulties, can one assert oneself.

"The Childhood Memories of Baron de la Motte Fouqué" by Dr. Emil Lorenz, and "From the Life of Guy de Maupassant," by Dr. Th. Reik (in *Imago*, II, pp. 513-521) show the neurotic constitution of both these writers, with the usual father-mother constellation.

In Karpas' article on Socrates, above mentioned, the well-known eccentricities of his character, including his "inner voice" and his indifference to his family obligations, are connected with homosexuality, shown by his love for young boys, and a specific "mother-complex," shown by his turning away from his father's trade to an avocation which he compared to his mother's, in that he called it "moral midwifery" or bringing about the birth of ideas in others.

In his article on "The Unconscious and the Dreams of Hebbel," Sadger declares that Hebbel anticipated a great deal of the knowledge of the subconscious that psychologists have lately found out. In the diary and letters we find observations about the unconscious mind, and about spirits. "Spirits are but symbols of our wishes and fears." "Shakespeare had to create murderers in order not to be a murderer himself." "In great poets things push up out of the chaos of their own force." "The insane are nearer to the other world than we." These are some of his pregnant sentences. He knew much, also, of the nature of dreams and used this knowledge in his works. "All dreams are perhaps only memories," and "when we sleep the god in us wakes" are among the writings in his diary. He well understood that dreams are wish fulfillments, and clearly saw the interdependence of dreams and poetry. Dreams of childhood in which he once more trembles before his father show his strong father-complex, and perhaps a consequent masochistic tendency. His writings abound in material that not only supports the Freudian teaching but that also is of great and lasting worth for the understanding of this poet's own genius.

In an article found in Volume I of *Internationale Zeitschrift für Ärztliche Psychoanalyse,* pp. 115-169, Sadger treats further of the neurotic features of Hebbel's case, and of their connection with sexuality. In a second article in Volume II of the *Imago* he takes up the work of Hebbel again, incidentally to a discussion of Pathography and Psychography. In this he analyzes Hebbel's drama, "Judith and Holofernes," and shows how the poet's conception of Judith as "neither a maid nor a wife, but a virgin widow," was traceable to his Oedipus complex. Judith, Hebbel declares, could never have done the deed had she been other than this—a woman married but widowed before she had known true marriage. The virgin's ignorance and the wife's experience would alike have been deterrent. This, pieced with facts of Hebbel's childhood, indicates to Sadger a mother-complex, part of which was the poet-child's inability to endure the thought of actual marital relations between his father and mother.

Carl Spitteler is another German poet hailed by the psychoanalytic school (Hans Sachs, *Imago* II, pp. 73-77), as one whose work supports Freud's conclusions as to the unconscious life. Sachs says, "His work is nothing less than a compendium for psychologists and neurologists." In a second article upon Spitteler, entitled "Homer's Youngest Descendant," appearing in *Imago,* 1914, Sachs tells us that Spitteler has given us the inner world as extensively and perfectly as Homer painted the external world. His works have given us the material that goes into the dreams of the average man—the life of a child, with the significant features evolved into powerful symbols, through which later experiences work.

The study of "Motive-formation in Schnitzler," by Hans Sachs, shows this dramatist's grasp of the principle of ambivalence, carrying the analysis through four or five of Schnitzler's works, and tracing the oscillation between love and hate in some of the characters in each.

In an article entitled "The Omnipotence of Thought in Schnitzler," by Dr. Theodore Reik, an article which is but an extract of a larger and uncompleted work on Schnitzler, it is shown that the author had a thorough knowledge of the mechanism of narcissism, or self-love, with its relation to the repression-neurosis developed in childhood, and the growth of the conception of the all-powerfulness of thought as a result of this neurosis and the narcissus-complex. It is the child, killing those who thwart his will in fancy, by the mere force of his wishes, who grows into the man with faith in the power of thought, of "mind over matter." Schnitzler's own infantile experience and subsequent conflicts must have taught him this.

No doubt Reik's larger [illegible] "Schnitzler as a Psychologist" will take its place am[illegible]ificant psychoanalytic studies of artistic genius.

Richard Adolf H[illegible] a study [illegible] Kant and Swedenborg (*Grenzfr*[illegible] *Seele*[illegible] 1909), which is of interest to the psychoanalyst because of the account of the abnormal traits of Swedenborg contained in the fourth part. It is shown that Swedenborg, in his later years, at least, was of a restless and nomadic temperament. He traveled constantly and seemed highly excitable at times. At other times he was in a trance-state, or a state of auto-hypnosis. He grew very ascetic, would eat no animal food, and was probably ill-nourished; moreover, he drank coffee constantly, and this might have affected his nervous balance. Whether there had ever been a sexual trauma it is difficult to know. Swedenborg was never married; he had, in youth, been betrothed to a young girl, but gave her up when he found that she did not love him. Some of his recorded dreams show sexual elements, as is to be expected. In his last years his speech was obstructed; he spoke very slowly and at times stuttered. All these traits suggest a partial dissociation of consciousness, or a very active *un*consciousness, but that all of Swedenborg's spiritualistic experiences are explainable by this the author does not claim. He thinks that though Kant may have been right in calling Swedenborg a candidate for the hospital yet the latter was able, in his ecstatic state, to perceive truths that must escape ordinary men.

A study of genius by an American writer, which, having been published in America, in our English tongue, scarcely needs an introduction to American readers, is "A Study of the Epilepsy of Dostojewsky," by L. Pierce Clark, published in the *American Journal of Medical Science,* 1914. I can hardly forbear, nevertheless, giving it a place in these summaries of the psychoanalyses of genius, since it treats a new phase of the subject in an able and significant way. Dostojewsky was one of those brilliant men whose genius is clearly allied with a psychopathic disposition. Some critics, among them Ossip-Lourie (in "The Psychology of Russian Novelists"), declare that he is not really a genius, and that his books owe their appeal to his power of description of suffering, which he owed to his own suffering. Clark, who draws largely from Segaloff's work on Dostojewsky, shows how very clearly the epileptic character is shown in Dostojewsky's life and work, how his work is moulded from the experiences of epileptic attacks and their prodromal symptoms and after-effects, how the books abound in psychopathic characters, everyone drawn from the personal experience of this very subjective writer, and how the heights and depths of human feeling sounded in the novels are transcripts of the ecstacies and black depressions of the epileptic sufferer.

Alienists have marveled at Dostojewsky's expert knowledge of psychopathology, exact and comprehensive, which was drawn from self-study. Strakoff states that all which Dostojewsky wrote had been lived through by him with fervor and devotion. In order to rank among the greatest of a great school of novelists Dostojewsky had but to write down, with the circumstantiality of the epileptic character, his own psychic experiences.

Clark, in his personality study of the epileptic character (*American Journal of Medical Science,* Nov. 1914), has found that the epileptic attack is a libidinous satisfaction to the epileptic individual, and the libidinous strivings are simple and infantile. Many epileptics have a feeling of ecstasy before the attack, for which, to quote Dostojewsky's own words, "though it last but five seconds one would give ten years of life." In such an instant of ecstasy Mohammed, also an epileptic, visited paradise, and returned. As Dostojewsky says, this was not a lie on Mohammed's part. Perhaps all our most widely known conceptions of heaven have been given us by epileptics. In these moments a peace, a harmony with all the world, an ineffable joy "that one could not bear longer and live" is felt. The anguish of the attack, the torpor following, and the deep depression, often accompanied by a feeling of guilt for a nameless, unknown, sin, are as deep as the ecstasy is high. All this Dostojewsky experienced, and showed as well the other epileptic symptoms of moodiness, deteriorating memory, fits of unreason, unsociability, and general intolerance. His book, "The Devil," written after the disease had begun to tell upon him shows these last named traits more clearly than the others. His memory had suffered so much by now that he could not remember what he had written and was obliged to read back; at the same time he hated the book and never re-read it after it was finished.

The causes for the formation of the epileptic constitution are as yet very imperfectly worked out. Dostojewsky had his first actual attack during his exile in Siberia. But before this he was neurasthenic, hypochondriac, and suffering from various physical disturbances of a psycho-genic nature. His childhood shows features likely to generate abnormal development. His fragile mother died when he was quite young, about the same time that Pushkin, whom he ardently admired, was killed in a duel. The boy suffered a great shock, and a loss of voice for several days, which was no doubt of psychogenic origin. His father was a military surgeon and a strict disciplinarian against whom the boy rebelled, as he rebelled against the discipline of the Engineering School later. That he was born with a psychopathic disposition there is no doubt, and there was little in his upbringing to correct this. He had throughout life the infantile type of mentality. "He reasoned or dated all things from his own inner consciousness. He was intensely egoistic, most of his reported conversations were but monologues on his part. He continued to develop his self-centered characteristics, failed to observe the conventions of society, did not recognize friends in his fits of abstraction, inconvenienced them by requiring their household arrangements to be changed to suit his own convenience." (Clark.)

He had childish tantrums and odd ways of defying the physician's prescriptions. Some time before his epilepsy developed his personality underwent further changes; he had fits of depression and anxiety, lethargic sleep after which he seemed to strive to continue his life in a dream. He also had periods of extreme irritability, embittered himself with governmental authority and suffered in lonely exile. It was after this revolt against the government (which to psychoanalysts is unconsciously a revolt against the father) that he lost his neurasthenic symptoms and became patient, tranquil, master of himself. (The writer has known similar sequences of events in other neuropathics. There is no doubt, at any rate, that a good fight in which one's own independence is asserted acts as a curative.) This means that the libido wins a freer range. How much of Dostojewsky's repression was permanently freed as he soon after developed epilepsy, another, and a pathological. method of self-emancipation, is a question.

Dostojewsky clearly perceived the infanti
mind as is shown in the portrayal of
Client," and in "Nelly," and some of hi
His power of self-analysis, added to his

tional experience, his psychic detachment from the normal world are responsible, it seems, for his power as a novelist, and these qualities are part and parcel of the epileptic constitution. He is not the only great epileptic. There is something to be learned from a psychoanalytic study of Mohammed, of Caesar, of St. Paul. But no one has yet told us why some epileptics and some neurotics are geniuses while others are helpless sufferers or dements. There are guesses, to be sure, but no satisfactory explanation.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. Titchener and H. P. Weld

XXVIII. Mechanical vs. Manual Stimulation in the Determination of the Cutaneous Two-Point Limen

By M. Carnes and L. C. Shearer

In the determination of the limen of dual impression upon the skin, great care is necessary in order to reduce the errors incident to the application of the aesthesiometer. The two points must be set down simultaneously and with the same pressure. Their rate of application should be controlled. If they are removed before judgment is made, the time of application should be constant and their simultaneous removal assured. The control of most of these factors is usually left to the manual skill of the experimenter. In instruments of the type of Washburn's[1] or Jastrow's[2] aesthesiometers a constant pressure (the weight of the instrument) is secured by allowing the handle to slip loosely upon the shaft which supports the points, so that the experimenter can not decrease or increase the pressure upon the points after they have been applied. This device does not, however, insure equal pressure upon the two points; the experimenter must keep the shaft of the instrument perpendicular to the skin, or he will exert more force upon the one point than upon the other. It seems then as if a mechanical applicator, which brings the points down simultaneously, with equal pressure, and at a constant rate, should constitute a more desirable method of stimulation.[3] Accordingly we arranged to put the question to the test of experiment.

The one of us (*C*) acted as observer, the other (*S*) as experimenter. With the old form of the Jastrow aesthesiometer,[4] we twice determined the longitudinal limen for an area of the volar surface of the left fore-arm, 5-10 cm. above the wrist; once with the usual manual application of the aesthesiometer, and once with mechanical application. We used the method of constant stimuli, taking one hundred series for each mode of stimulation. The series for mechanical and for manual stimulation we divided into groups of ten, and alternated the groups (with reversal of alteration after the first hundred and the interval of a day between the two hundred) so as to equalize practice. In order to avoid fatigue frequent rests were introduced. Not more than thirty series could be taken at a session, and frequently it was found advisable on account of fatigue to stop after twenty series. Every series consisted of five separations, and the same values (2, 8, 14, 20, and 26 mm.) proved to be suitable in each case.

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[1] M. F. Washburn, Some Apparatus for Cutaneous Stimulation, *Amer. Jour. of Psychology*, 6, 1894, 422.

[2] The improved form is described by G. M. Whipple, *Manual of Mental and Physical Tests*, 1914, Pt. I, 246.

[3] An editorial note in *Amer. Jour. of Psychol.*, 1, 1888, 552 f., describes a fixed form of Jastrow's aesthesiometer in which the position, but apparently not the rate of application, is kept constant. The writer cites no experimental results to demonstrate the advantages of the instrument. See also E. W. Scripture, *Thinking, Feeling, Doing*, 1895, 111.

[4] Similar to the improved form used by Whipple, *loc. cit.*, but shorter and with pyramidal points. Its weight (without the handle) is about 27 grams, giving a pressure on each point of 13.5 grams. It is figured in the catalogue of the Garden City Model Works, Chicago, 1894. See E. B. Titchener, *Experimental Psychology*, II, ii, 1905, 160, 258.

The mechanical application of the stimulus was secured by means of Titchener's applicator. The handle of the aesthesiometer was fixed to the end of a hollow, vertical brass rod; the shaft passed up through the rod. The rod bore a rack upon its side, and could be lowered through a bearing by turning a crank which operated a set of gears (worm, wheel, pinion, and rack). A single revolution of the crank lowered the rod 3.25 mm. The rate of application was controlled by turning this crank in time with a soundless metronome, at the rate of one revolution per sec.; a linear speed of application of 3.25 mm. per sec. Two complete turns brought the aesthesiometer free upon the arm. The observer lay supine upon a couch beside the apparatus. His arm was extended, volar surface upward, beneath the apparatus, and was held in a plaster cast. The cast enclosed the arm on three sides and was provided at the end with a grip, moulded to the hand, which the fingers grasped. It was adjusted beneath the apparatus and fixed so that a straight line upon the skin lay horizontally and directly under the aesthesiometer. The limen was determined along this line. To prevent the fatigue of particular spots provision was made for shifting the whole stimulating apparatus a little distance along the line; the casting, which bore the gears and the bearing for the vertical rod, was arranged to slide on two horizontal rods. A long wooden screw at one side furnished a means for moving it horizontally. The experimenter turned this screw before commencing a new series, thus moving the apparatus along the arm and preventing too frequent stimulation of the same spots. After several series, the apparatus was set in its initial position and again shifted along. Since the simultaneous application of the two points was assured by the apparatus, and since the aesthesiometer could move only in a straight line, the pressures upon the two points were equal (*ca.* 13.5 g. each). We found that the arm was not perfectly rigid within the cast and that an alteration in the position of the body at successive sessions sometimes threw out the adjustment of arm to apparatus. It was always possible, however, either by moving the body or by shifting the apparatus slightly in the horizontal direction, to secure again the desired adjustment.

In the series with manual stimulation the arm was kept in the cast and the position of the observer was unaltered. The experimenter worked always with the utmost care.

The following percentages of 'two-judgments' were obtained:

Separations:	2 mm.	8 mm.	14 mm.	20 mm.	26 mm.
Manual application	16%	37%	62%	87%	97%
Mechanical application	11%	37%	64%	84%	94%

From these percentages the limens were computed by means of Urban's tables.

Manual application: Limen = 11.4398 mm. $h = .0816$
Mechanical application: Limen = 10.7894 mm. $h = .0837$
Difference = 0.6504 mm.

Mechanical application gives both a lower limen and a higher value of h. The difference in favor of mechanical stimulation is not great (6%); yet it is significant. Its probable correctness (*i. e.*, the probability that the manual limen is greater than the mechanical) is 83%.[5]

Conclusion.—We conclude, then, that there is a slight scientific advantage gained by the mechanical application of the aesthesiometer. The determination of the limen is slightly more accurate, and the limen itself is slightly lower—as a result, we may assume, of the greater evenness of stimulation—than with manual application. The differences, though small, are mathematically significant. The advantage is, however, too slight to require the use of a mechanical method under the usual conditions. For all practical purposes, careful manual stimulation is adequate.

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[5] For the method of computing this probable correctness, see discussion by E. G. Boring in the present number of the JOURNAL.

XXIX. ON MEMORIZING WITH THE INTENTION PERMANENTLY TO RETAIN

By F. P. Boswell and W. S. Foster

That the intention to retain is an important factor in memorizing has been assumed from the very beginning of the experimental investigation of memory. Ebbinghaus says specifically: "During the process of learning, the purpose of reaching the desired goal as soon as possible was kept in mind as much as was feasible. Thus, to the limited degree to which conscious resolve is influential in accomplishing it, I endeavored to keep attention so far as possible concentrated on the tiresome task and its purpose."[1] This rule of Ebbinghaus has been followed with few exceptions by all later investigators. Poppelreuter remarks that *all* previous memory experiments have favored the *intentional* formation of associations. He can reproduce a series of 12 nonsense syllables after 12 repetitions; but when as experimenter he merely reads the series aloud, 50 repetitions enable him to recite only a few of the syllables.[2]

It has often been pointed out, moreover, that less general determinations are sometimes induced, either by instruction or by the observer's knowledge of the conditions of recall, and that these specific determinations influence greatly both the mode of learning and the adequacy of retention. Meumann,[3] for example, followed every presentation of his series with a test by the method of right associates, continuing the repetitions until 100% of correct responses were given. He then asked for a reproduction of the entire series, accented and unaccented syllables alike. His observers could recall none (or only a few) of the *accented* syllables. They had depended upon the experimenter for a re-presentation of the accented syllables, and their intention in learning had been directed exclusively upon memorizing the second members of the pairs. Indeed, by tests in which the syllables were presented individually at haphazard, Meumann found that many of the accented syllables were not even recognised.

The specific determination with which we are concerned in this paper is that of learning for permanent retention as opposed to that of learning for merely temporary recall. The generally accepted belief that 'cramming' (learning merely for the present) leads to inferior retention tends to receive incidental confirmation in the results of various experimental investigations. Aall, however, first made a special study of the effect of the intention permanently to retain.[4] A number of his experiments had a doubtful outcome, and he bases his conclusions chiefly upon mass-experiments with Norwegian school children. Even here the expected result did not appear in all cases.

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[1] H. Ebbinghaus. *Ueber das Gedächtnis*, 1885, 34. For a pre-experimental reference, see W. S. Jevons, 'Cram,' *Mind*, ii., 1877, 198.

[2] W. Poppelreuter. *Zeitsch. f. Psych.*, 61, 1912, 3, 6.

[3] E. Meumann. *Zeitsch. f. päd. Psych.*, 13, 1912, 456ff.

[4] A. Aall. *Ber. über d. V. Kong. f. exp. Psych.*, 1912, 237ff; *Zeitsch. f. Psych.*, 66, 1913. 1ff.

Two stories, each with a decided 'point,' and two groups of familiar objects were used as material. Certain classes were told that an examination on the material would be held next day; others that the test would come after several weeks. On the second day the first class were told that something had prevented the completion of the experiment, and that the test was 'called off.' At the end of 4 or of 8 weeks both groups of classes were tested. Measured in terms of errors (falsifications and omissions), in the case of the stories the second group showed 4-13% better retention. Measured in percentage of retained objects, the second group retained 18% better in one series of experiments, and 7-9% in another (one contradictory class not counted).

Having in mind Aall's failure to find conclusive results and his neglect to study the influence of intention upon the mode of learning as well as upon retention, we have attempted a preliminary study to discover, if possible, a more reliable and more specifically analytical method. We planned not merely to measure the capacity of our observers for reproduction, after learning with the different intentions, but also to test recognition, and to study certain aspects of the processes of learning and of recall. We had in mind the expressions of Meumann and of G. E. Müller with regard to differences which might be expected in these latter regards.[5] Meumann believes that a difference of attitude is involved in the two cases, and that an observer naturally seeks to make sensible connections in order to favor permanent retention. Müller finds that the sense-modality of learning and of reproduction may shift as a result of knowledge of the length of interval which is to elapse between learning and test.

Our experiments were made in the Psychological Laboratory of Cornell University during the Summer Session of 1915. The observers were Miss G. English (E), candidate for the Master's Degree in psychology; Miss M. Wright (W), whose training in experimental work was gained in the two elementary laboratory courses; Mr. F. L. Dimmick (D), assistant in Psychology and candidate for the doctorate; and Miss N. P. Lawson (L), whose experimental training was limited to a single laboratory course. Aside from a single practice-series, our material consisted of a Chinese-English vocabulary of 128 pairs of words arranged in 8 series of 16 pairs each. The words were typewritten in the usual English characters, and presented in the Spindler and Hoyer *Gedächtnisapparat.* The Chinese word on the right and its English equivalent to the left appeared simultaneously, and remained visible for 2.5 sec., with a blank space at the end of every revolution of the drum. The observers read aloud in trochaic rhythm for sixteen continuous repetitions. After a five-minute interval, the Chinese words were exposed in haphazard order, and the observers were required to give their English equivalents. They were in every case asked to report, if possible, the nature of the reaction. Although instructed that it might well be impossible to state in every case whether the reaction was or was not preceded by other processes than those concerned with the apprehension of the stimulus, in nearly all cases they felt able to do so. Furthermore, if imagery relevant to the recall appeared, they nearly always felt able to indicate briefly its character. The times of reaction were taken with a stop-watch; but the observers were instructed specifically that they might take all the time they considered advantageous for making a correct recall. Each observer at the given hour learned two series on each of four successive days; one series for temporary (T), the other for permanent (P) retention. Ten minutes elapsed between the test of one series and the learning of a second. It was explained that we were interested especially to see if the mode of learning and of recall under these two instructions was alike or different; and both P- and T-series were tested after five minutes with this comparison in view. In the T-series the observer was instructed to give fullest possible attention, and to make all possible endeavor to master the series so completely as to attain a full 100% of correct replies after five minutes. In the P-series the observer was told to learn for permanent retention; although for the sake of comparison a test would also be made after five minutes, the real test would be given about two weeks later, and this would serve to show how well the task had been accomplished. The usual warning to think as little as possible of the words between learning and recall was given.

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[5] G. E. Müller. *Zeitsch. f. Psych.*, Ergbd. 5, 1911, 16ff. E. Meumann, *loc. cit.; The Psychology of Learning* (Trans.), 1913, 74ff.

After an interval of two weeks, at the hour of the original learning, series composed of the Chinese words from both the P- and the T-series mixed at haphazard were again presented, and the English equivalents were again required. In this test the same sort of introspective report was required as in the earlier tests; and in addition a report as to the familiarity or lack of familiarity of the stimulus-word.

E found no essential difference in her mode of learning under the two instructions. She simply adopted what she believed to be the 'natural' plan of making a sensible connection wherever possible, between the words of a pair, and of trying to anticipate the English words as soon as she had attained a certain degree of mastery of the series. Questioning after the end of the experiment brought out the fact that she had been extremely interested in the work throughout, not merely for its own sake, but also because she expected later to become a missionary worker in China. She had not interpreted the instructions to mean that permanent mastery of the T-series was forbidden.

W was disturbed by the task of learning for permanent retention. Its difficulty appeared to her very great, and she lacked self-confidence in the face of it. She felt that she did not "know how to go to work to impress the series permanently." She reports that she gives "tense attention" in learning this series, but that "attention isn't so constant and easy;" that "she can't help emphasizing some parts." In the T-series she does not "feel under so much strain," although she gives "just as much attention;" and she feels freer to try a greater number of anticipations. Here also she tries to establish more place associations, feeling that these may be advantageous after five minutes as they would not be after a much longer period.

In learning the T-series D has more regard for making definite the "feeling of saying the words of a pair together," or for "making the recitation fluent;" in the P-series he endeavors to form more meaningful connections between the words of the pairs, because he feels that he can "retain meaningful connections longer than the mere vocal-motor feel of the two words." In the T-series he pays less "attention" to the Chinese word. With greater "attention" to the English word he can risk the stimulus word touching it off, if the test is made soon. In the P-series he "distributes his attention" more equally to both members.

Even more than D, L seeks to form meaningful connections in the P-series. Like E she tries also to form place associations, especially for temporary retention, and seeks to support learning by emphasizing the rhythm of speech, by beating accents with her fingers, etc. In both series she continually seeks to anticipate in auditory and in kinaesthetic terms, the latter both vocal and graphic.

Our observers themselves, however, were impressed not so much by the differences as by the similarities of learning in the two series. The reports which we quote are selected to emphasize such differences as could be found when we looked especially for them. It is probably a mistake to require a change from one task to another within the limits of a single hour, if it is desired to give the freest opportunity for shifts of attitude and procedure. In experiments upon constrained association Watt, e. g., found that observers tend for some time to react in the sense of a just previous requirement (*Perseverationstendenz der Aufgabe*);[6] and Müller mentions a tendency of the mode of learning to persist (*Beharrungstendenz der Lernweise*).[7] We conceive that our failure to find constant and gross differences in the recall of the two series may possibly be due to this fact.

Table I shows the results of the tests after five minutes. The actual number of correct replies is designated by *r*, the number of wrong replies by *w*, the number of cases in which no reply was given by *o*. *Tr*, *Tw*, and *To* are averages of the corresponding reaction-times, expressed in seconds.

TABLE I

	Obs.	*r*	*Tr*	*w*	*Tw*	*o*	*To*
Temporary	E	59	3.0	1	40.0	4	38.5
	W	61	2.9	1	15.0	2	24.0
	D	52	5.0	6	8.2	6	15.5
	L	59	3.5	2	3.0	3	41.3
Permanent	E	61	2.5	1	2.0	2	42.5
	W	53	2.7	3	7.7	8	32.1
	D	47	3.4	3	16.0	14	16.3
	L	58	2.7	4	9.0	2	46.5

It is evident that 16 repetitions were sufficient to give adequate mastery of the vocabulary for such immediate recall, although no observer failed to make a few mistakes, and none of them, we feel assured, became overconfident or inattentive in the learning period. Two observers (W and D) show decidedly the advantage of the T-series over the P-series in number of correct recalls. In the case of W the advantage is to be noted especially, both because of its magnitude and because it bears out her report that the task of learning permanently was disturbing. The slight advantage of L's T-series and the slight disadvantage of E's are probably insignificant. The average times of the *r*-replies in the T-tests are the larger. As a possible explanation we suggest that in this test the observers may have placed greater emphasis upon giving a full 100% of correct replies; but we realize that many other factors are probably involved. The average times of the *w*- and *o*-replies are computed from so few cases that they are probably not significant. Although in the Table the average *o*-time for every observer is greater in the P-series, yet if we average the individual times for all four observers we get a higher average for the T-series (27.9 as against 25.8).

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[6] H. J. Watt. *Arch. f. d. ges. Psych.*, 4, 1905, 343ff.

[7] G. E. Müller, *op. cit.*, 19ff.

Since two of our observers were only moderately skilled in introspection, we should probably not place too great dependence upon the accuracy of their reports of the manner in which the reply-word appeared. We have made an analysis of the reports for the *r*-cases, however, and find certain tendencies which we believe to be significant. In by far the greatest number of cases in both T- and P-tests, the mere apprehension of the Chinese word (or its repetition in internal speech) seems quite immediately to touch off the appropriate response. The observers call these cases "vocal-motor reactions." This mode of recall is especially noted in the T-series, where 73% of all recalls were of this type. For the P-series the corresponding percentage is 64. The difference between the two series does not appear with E, but is evident in the cases of W (72% *vs.* 57%), D (86% *vs.* 78%), and L (68% *vs.* 47%). As between the two series, no significant differences appear in the modality of imagery, or in the frequency of occurrence before reaction of place-ideas and of ideas representing meaningful connections. As between observers, however, certain differences occur. E reports for the most part verbal and visual ideas representing sensible connections, W visual ideas representing place, D visual ideas of place (the reaction often being followed by verbal ideas representative of the sensible connections), and L chiefly auditory and kinaesthetic (graphic) ideas.

Table II shows the qualitative results of the tests after the two-weeks interval. The symbols have the same meaning as in Table I.

None of our observers realized that a second test of the T-series would be made after the two-weeks interval, although it occurred to

TABLE II

	Obs.	*r*	*Tr*	*w*	*Tw*	*o*	*To*
Temporary	E	22	9.1	2	28.5	40	42.3
	W	9	7.7	19	11.7	36	34.5
	D	18	8.1	1	3.0	45	20.3
	L	41	4.4	11	4.5	12	6.5
Permanent	E	20	18.3	5	25.8	39	46.1
	W	7	8.3	17	20.0	40	35.0
	D	21	10.3	3	7.0	40	17.9
	L	48	5.5	6	4.3	10	5.6

D at one time that such a test might possibly be required. Requiring the observers to introspect upon the mode of learning and the mode of recall, and giving them definitely the impression that we were concerned to discover possible differences between the two series, we seem effectively to have prevented the presence of any suspicion, at least during the learning. W and L indeed did not realize until fairly late in the test that words from both series were being presented.

E now makes slightly more correct recalls in the T- than in the P-series. The explanation is, first, that she did not learn the T-series *solely* for temporary retention, and her mode of learning in the two cases is much the same; and secondly that, being interested in retaining the whole vocabulary permanently for an external purpose, and taking it for granted that after the five-minute test this series was of no account experimentally, she allowed herself in the two-weeks interval to repeat a number of the associations.—Striking indeed is the small number of correct recalls made by W. Although in the test after five minutes she had shown a definitely better mastery of both series than D (51 *vs.* 47; 61 *vs.* 52), she now is able to give no more than one-third to one-half as many correct replies as that observer. Thus her report that permanent retention is hard for her to plan for, and learning for permanent retention definitely more disturbing, is again reflected in the objective results.—The *r*-replies of D show a slight advantage in favor of the P-series, and those of L a considerably greater one.

W's large number of *w*-cases also fits well with the report that in learning she was confused. The *w*-cases are also interesting from another point of view. Those of E in the T-series were neither relevant in meaning, nor did they belong to the same vocabulary-group of 16 pairs as the stimulus word; two of the five wrong replies of the P-tests were relevant. None of W's 19 *w*-replies in the T-series were relevant, and only one belonged to the appropriate vocabulary-group; whereas of 17 in the P-series, 5 were relevant and 4 belonged to the group. One of D's 3 *w*-replies in the P-series was relevant and one other belonged to the appropriate vocabulary-group; his single *w*-reply in the T-series had no relation to the learning. Of L's 11 *w*-cases in the T-series 2 were relevant and 8 belonged to their appropriate vocabulary-group; of 6 in the P-series 2 were relevant and 3 belonged to their vocabulary-group. In this connection we may mention that an analysis of the rejected replies in the *o*-cases points similarly to a greater retention in the case of the P-series. Relevant or correct words and other relevant ideas occurred, in the T-series, to E in 15%; to W in 8%; to D and W in 0% of the *o*-cases. In the P-tests, the corresponding figures are 7, 31%; W, 13%; D, 5%; L, 20%.

Of the Chinese words presented in tests of the T-series 73% were familiar to E, 63% to W, 73% to D, and 77% to L. In the P-series the corresponding figures are E 81%, W 63%, D 77%, and L 86%. The tendency, therefore, is toward greater familiarity with the words of the series learned for permanent retention, although the tendency is generally slight.

In the P-cases the average times of correct reaction, as might be expected, are longer than in the test after five minutes. The averages in the T-tests are 1.1—2 times as long as those of the P-tests, and the difference between the times of reaction after five minutes and after two weeks is much more marked in the former series (2-7 as against 1.3-3 times as long). The reports indicate very definitely a falling off in the percentage of immediate or 'vocal-motor' reactions with the longer interval. The T- and P-series differ a little in this respect, the former still showing slightly the greater percentage, 57 as against 52%. Again we place little dependence upon the exact percentages. It seems likely to us that, in view of the length of the reaction-times, unreported sensory or imaginal processes not connected with the mere apprehension of the stimulus-word may well have occurred, and under the unfavorable conditions may have been overlooked. More frequently than in the earlier tests, the occurrence before reaction of imagery of the reply-word or of imagery referring to the learning is reported. For the most part the latter references are to the sensible connections. In only 2 cases (as opposed to 75 in the earlier test) are definite place associations reported. Of the observers L most frequently reported merely the imagery of the reply-word itself in auditory, kinaesthetic-vocal and graphic terms. D, W, and E report about equal numbers of rejected words, many more than does L. The references to sensible connections made in learning are most frequent with D and E. They are most frequently carried by visual and verbal ideas. (*Cf.* the relatively long times of these observers.) W and L ordinarily simply repeat the stimulus-word in inner speech, until some English word appears and is recognised and thought to be correct. L often reports a reference to the sensible connections made in learning, but most frequently these references appear as confirmations rather than as inducing factors of the reply.

As stated at the outset, we regard these experiments as useful primarily on the side of orientation in method. More reliable results would have been obtained if our observers had been given longer preliminary training in memory-work, a greater number of series, instructions of only one kind in a single hour, and the other advantages which we have mentioned. In so far as our results bear upon the problem attacked, they may be considered to show that *the intent to learn for permanent retention really brings about the desired end in the case of learning a vocabulary.* If we measure retention solely by the number of correct replies, two of our observers, to be sure, do not confirm the theory. Both cases are to be explained, however, by factors irrelevant to the question at issue; and the results of these observers should therefore be discounted, so far as the general question is concerned. Even if the correct replies of all observers are simply added together, the series learned for permanent retention total 96 to 90 in favor of the expected result, whereas after five minutes the reproductions stand in the opposite order, 219 to 231. If to this evidence we add the facts that, in the P-series, the *w*-cases are more frequently relevant in meaning or are words from the same vocabulary-group as the stimulus, that in the *o*-cases more correct or relevant replies are rejected, and that in general a slightly greater familiarity is shown, the supposition is considerably strengthened.

We find that the effect of the intention is not altogether immediate, so far as retention is concerned, but that it becomes effective in part at least through its *influence upon the mode of learning.* We have to some extent confirmed Meumann's and Müller's observations in this respect. With our material, the intention to retain permanently tends in the more objective observers to induce a greater emphasis upon the establishment of meaningful connections while learning, and in different cases to lead to (or to emphasize) other devices not so apparent in learning for merely temporary recall. These differences in mode of learning are reflected also in the nature of the recall and in the reaction-times.

XXX. SOME USES OF ARTIFICIAL DAYLIGHT IN THE PSYCHOLOGICAL LABORATORY

By A. J. Brown

The use of daylight in matching colored objects or surfaces is unsatisfactory, because daylight varies throughout the day and on different days in composition and intensity, thus altering both hues and tints. Artificial light possesses an advantage in that both its intensity and its composition can be controlled. Sources of artificial daylight, which duplicate certain conditions of natural daylight and which thus furnish desirable constant conditions for work with colors, have recently been developed by Luckiesh[1] and by Gage.[2] There is no reason why the psychologist should not avail himself of these sources of illumination.[3]

We have recently compared in the Cornell Laboratory color-matches under ordinary 'laboratory' daylight with matches under illumination by an 8⅜-in. roundel (convex, acid-etched) of Gage's glass used with a 100-watt nitrogen-filled Mazda lamp.[4] The conditions were those of the ordinary undergraduate experiment in color-mixing. Hering B and Y and Milton-Bradley G and R paper were matched to gray (Bk-W). The matches were made in a dark room; the daylight matches with the window-shutters open, the matches under artificial light with the lamp placed above and in front of the mixer. The results, in degrees, for 14 observers (all practised in color-mixing) are as follows:

RED-GREEN TRIALS

NATURAL DAYLIGHT					ARTIFICIAL DAYLIGHT				
Outside			Inside		Outside			Inside	
R	G	Y	Bk	W	R	G	Y	Bk	W
215	145	...	274	86	216.5	153.5	....	287	73
210	150	...	270	90	215	145	....	294	66
207	153	...	263	97	212	148	....	285	75
211	149	...	275	85	206	144	10	280	80
220	140	...	275	85	225	135	....	296	64
222.5	137.5	...	288	72	224.5	135.5	....	295	65
212	148	...	275	85	219	141	....	283	77
207.5	149.5	3	272.5	87.5	215.5	140.5	4	285	75

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[1] M. Luckiesh. *Color and Its Applications*, 1915, 224-251, 302-311.

[2] H. P. Gage. "Daylite Glass," Sibley Journal of Engineering, 30, 1916, 247-250.

[3] We are here speaking of the routine-work of the laboratory, and not of such research as requires a special optics-room (see C. E. Ferree and G. Rand, *Psych. Rev.*, xix, 1912, 364ff).

[4] The outfit is described by Gage, *op. cit.* Th [illegible] reflector, *etc.*, without lamp) can be bought [illegible] roundel alone for $2.75.

	212	148	...	272	88	210.5	145.5	4	284	76
	218	142	...	274.5	85.5	218.5	141.5	....	290	70
	194	157	9	268	92	200	152	8	278	82
	211.5	144.5	4	271	89	215	141	4	287.5	72.5
	210	145	5	267	93	211	139	10	288.5	71.5
	208	148	4	268	92	212	144	4	278.5	81.5
Av...	211.3	146.9	1.8	272.4	87.6	214.4	142.5	3.1	286.5	73.5
M. V.	4.5	3.9	2.3	3.9	3.9	4.9	3.5	3.1	4.6	4.6

BLUE-YELLOW TRIALS

	NATURAL DAYLIGHT					ARTIFICIAL DAYLIGHT			
	Outside			Inside		Outside		Inside	
	B	Y	R	Bk	W	B	Y	Bk	W
	193	167	...	210	150	192.5	167.5	216	144
	191	169	...	210	150	187	173	208	152
	190	170	...	205	155	185	175	207	153
	191	169	...	207	153	191	169	199	161
	187	173	...	202	158	186	174	190	170
	192	168	...	202	158	189	171	197	163
	190.5	169.5	...	205	155	189	171	202.5	157.5
	187	173	...	212	148	192	168	202	158
	188.5	170.5	1	205	155	190	170	197	163
	186.5	173.5	...	212	148	192	168	202	158
	191.5	168.5	...	205.5	154.5	190.5	169.5	195.5	164.5
	188	172	...	198.5	161.5	187.5	172.5	191	169
	192	168	...	206	154	188.5	171.5	204	156
	190	170	...	202	158	186	174	190	170
Av..	189.2	170.8	....	205.9	154.1	189.0	171.0	200.1	159.9
M. V.	1.9	1.9	....	3.1	3.1	2.0	2.0	5.9	5.9

The differences in the average amounts of Bk and W under the two illuminations are probably to be accounted for by the fact that the daylight illumination was more intense than the artificial.[5]

The differences in the average amounts of color are not great. In the R-G trials (this pair of papers is the more difficult to match), the differences are 3.1°R. 4.4°G, and 1.3°Y (some observers required a little yellow). In the Y-B trials, the differences are 0.2°Y and 0.2°B. In the R-G case the differences between the two illuminations are comparable to the M.V. among observers under either illumination; in the Y-B case, the differences are negligible. Certainly no violence in the way of alteration of hues would be done to the undergraduate experiment in color-mixing if it were performed in the dark room under artificial daylight; whereas the very great advantage of constant illumination would be secured. No one who has witnessed the discouragement of the careful student whose color-equations fail to check in the waning light of the afternoon will underestimate the desirability of light of constant intensity for such work. Not only the color-mixing but also the contrast experiment, and many of the psychophysical experiments upon brightness, would be facilitated by the use of artificial daylight.

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[5] Cf. L. R. Geissler, this JOURNAL, xxiv, 1913, 178.

We have also tried out this light as a possible means of color-demonstration for lectures in the evening or in poor daylight. In the usual illumination of the large lecture-room in the Cornell Laboratory in the evening it has not been possible to use the various colored charts and models, painted in oils: the color-pyramid, the color-square, the psychological spectrum, the chromatic scale of blues, and the two typical spectra of color-blindness. The blues fade into light grays, the greens almost disappear in dark grays, and the reds become orange. With artificial daylight the colors are restored. It is not even necessary to turn out the yellowish lights of the room. If the charts are placed directly beneath the source of artificial daylight, the colors are brought out as soon as this light is turned on, and are not noticeably altered by turning off or on the other lights of the room. The effect is, in fact, much pleasanter if the general yellowish illumination of the room is allowed to remain unchanged when the artificial daylight is thrown on the charts; for then the demonstration does not contrast strongly in brightness with its surroundings. For long charts (over 4 feet) two such light-units as we used are necessary. Especially is this true for the chromatic series, where the blues fade readily into grays. Two such units will illuminate the color-pyramid so that it can be seen, in true colors and without shadows, from all parts of the room.

The artificial daylight is equally satisfactory for other color-demonstrations, such as the exhibition of the matches of worsteds made by color-blind persons. We found also that a single light-unit could be used with the Ragona Scina apparatus for the demonstration of color-contrast. When some of the thinner glasses are used in the apparatus, and the light is placed in front and to one side of the apparatus-box, a contrast-effect is produced which is visible at a greater distance than is ordinarily the case in daylight.

XXXI. ON THE PSYCHOLOGICAL RESPONSE TO UNKNOWN PROPER NAMES

By G. English

In this paper we report some experiments which bear upon the nature of the psychological response to proper names of unknown persons. Kollarits, who was concerned with imagery only,[1] found (1) that such imagery is conditioned upon some personal trait or characteristic, such as literary style, religious, political, professional life, etc.; or (2) that the image is that of a person of the same or a similar name, or (3) of the same nationality. He suspects other factors, however, and he requests the testimony of other observers. Claparède has responded with a suggestion which we shall consider later. The problem is of interest not only because it involves the imagery of imagination, but also because it has a bearing upon the psychology of meaning. We have substituted experimental conditions for the more or less casual observations of Kollarits, and we have further submitted the suggestion of Claparède to experiment.

I. We first prepared a series of names of distinguished psychologists, and presented each name to four men and three women students in an advanced course in systematic psychology. The names were familiar, but the great majority of the persons were unknown to all. We hoped in this series to obtain records of the habitual imagery, if such imagery existed, which represented the names. The procedure was as follows: The observer sat with eyes closed, and was given a warning signal before the name was presented. The stimuli were given auditorily. The observer was informed of the general nature of the experiment, and was told not to construct a visual image, but if one came to describe it as fully as possible. It was soon discovered, however, that some observers had no habitual visual imagery; and we therefore repeated the series with the instruction to report what the person named 'must look like.' We hoped by this second instruction to stimulate visual imagery in those observers with whom it was not habitual. This attempt was on the whole a failure; and we have therefore combined the results obtained from both instructions. We give them in numerical form in the following Table.

TABLE I

Memory image of	
The person as seen	4
Picture (of the correct or of some other person)	14
Book	14
Apparatus	13
Known persons (little or no connection)	17
	62

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[1] J. Kollarits. Observations de psychologie quotidienne, *Arch. de psychol.*, XIV, 1914, 225-240.

Imaginary images of the person (unanalysed)	13
Picture and imaginary details	9
Images derived from	
Description of the person	2
Facts known about the person	10
Facts *plus* picture	2
Suggestion of nationality in name	4
Association (*Galton*, thin and wiry, like tones)	1
Sugestion by meaning of word (*Stout*)	1
Suggestion by sound of word (*Fechner*, thin word, thin person)	1
Name and literary style	1
Very vague imagery	6
Scrappy images in an attempt to get imagery	3
	53
Cases of no visual imagery	
Association to psychology	21
Verbal and miscellaneous associations	19
Internal-speech repetitions or visual image of word	36
	76

The results, when imagery was found, are quite similar to those of Kollarits. In only one-third of the cases, however, did visual imagery appear; in more than a third the associations were non-visual; and in many cases the proper name served only as a tag for a bit of apparatus or a book.

II. In the hope of avoiding memory-images and set meanings, we next selected from an English scientific journal a number of names which we thought would be entirely unfamiliar, but which should give the impression of actual names. Only one or two of the list proved to have been heard before, according to the observers' reports. Eleven observers, ten of them women, took part in the experiment; six of the eleven had had psychological training; the remainder were students specialising in literature. The series was given twice, under different instructions. Under the first, the observer was asked to report the mental processes which came involuntarily; under the second, he was asked to try to get an image of the person; if he could not, he was to say what the person 'would look like.' The second instruction yielded a larger number of visual images than the first; but qualitatively the results are so similar that we have for convenience combined them in the following Table.

TABLE 2

Imagery of persons	
Of same name	6
Of similar name	52
From verbal (chiefly auditory) association	54
From sound of name	6
With no reported associations	85
Suggested by nationality	7
Enumeration of characteristics (no visual imagery)	32
Verbal repetition (or nothing at all)	57
Verbal (chiefly auditory) associations	87
Miscellaneous	23
Total number of cases of vis	210
Total number of cases of no	9

In general these results are similar to those of the first series. There are, of course, practically no memory-images. There is a larger proportion of cases of visual imagery (½ as against ⅓), but there is still a large number of verbal reactions. The factors involved in the visual imagery are those which Kollarits has already pointed out. Only in six cases is there anything new: the images suggested by the sound of the name. A single image of this sort has been noted in Table 1.

III. This new result leads us to a consideration of the suggestion of Claparède noted above. He says: "The physiognomy of the proper name certainly plays a part. The sound of the name has an affective tone which co-operates in the elaboration of its mental representation. Other things equal, names consisting of heavy or repeated syllables call forth images of fat, heavy-set, bloated, or slightly ridiculous individuals; a short and sonorous name, on the other hand, suggests slender and active persons, etc. M. Patapoufard would evidently be of a type quite different from that of M. Flic. . . . It is not without intention that Daudet has created the name of *Tartarin,* Dickens that of *Pickwick,* Flaubert those of *Bouvard* and of *Pécuchet.*" The mental imagery aroused in these cases is not conditioned upon the individual circumstances and antecedents of the reader, "since these names produce a similar effect upon *all* readers."[2]

In the six cases cited above, *Klemm* was reported as a 'sharp word,' 'a thin word;' *Ponsonby* as 'a heavy word,' 'a ponderous name,' with 'a feeling of quick heaviness;' and *Dendy* as a 'thin, slight name.' In all these instances, as in that of *Fechner* in Table 1, the visual imagery evoked was in Claparède's sense physiognomic. The cases are, however, few in number; and since it was possible that the names chosen for the experiment were not suitable to arouse associations of this sort, we devised two further experiments in order to test Claparède's contention. In the first we constructed fifty nonsense-names of one, two and three syllables; a large number of representative vowel-sounds and consonant-combinations were put in a box, and the words were combined by chance. The names thus obtained were presented auditorily to eight observers; each name was pronounced three times over, the experimenter being careful to pronounce it slowly, distinctly, and (as nearly as possible) always in the same manner. As a check upon the auditory perception, the observer was asked to spell the word. He was further instructed to report his imagery or, in case visual imagery were wanting, to describe the person that 'must belong to the name.' In the second experiment, pen-and-ink drawings selected from back numbers of *Puck,* and representing various types of individual, were mounted singly on white cardboards and used as stimuli. These pictures were presented one at a time to each of seven observers, all of whom had taken part in the preceding experiment. The instruction was as follows: "I shall show you the picture of a person. You are to name that person suitably; you are free to use real or nonsense names. After you have named the picture, you will be asked for introspections."

If we take the 500 reports as a whole, the results are negative. *There is no constant or uniform tendency among these observers either to imagine a similar type of individual for the same name, or to furnish a similar type of name for the same picture.* Individual differences among observers are great. In the nonsense-series one observer failed completely; he was able only to suggest a nationality to which the name might belong; and he did not complete the series. Of those remaining, five depended mainly upon auditory association; thus, the name Daw′thō suggested *Delphos, doll, doddering* and *Daudy* (a friend); and the images were a Greek man, a doll-like girl, an old woman, and a memory-image of a woman. The two remaining observers tended to furnish images corresponding with the sound, although they were not consistent. One of them had as an image for Daw′thō 'a heavily built, strong Greek, very jolly;' and said in his introspective report: "It seems as if the sounds aw and ō, being generous sounds, should go with broad shoulders and perhaps good nature." In only five cases was there anything like agreement among all observers as to sex or other characteristics. Rŭpzóiyăt was reported as a young man by all observers; Bŏp′pŭm was said to be a tall, fat or large man by six observers (*Bottom* was associated, however); five thought Zē′thē must be a girl; six reported Grĭb as a small man; and five reported Kĭrd′faumĭsh as a strong or big man. For all the remainder there was disagreement.

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[2] *Op. cit.,* 301 f.

In the picture series, as a rule, three factors were operative in furnishing names: (1) similarity to some known person, (2) similarity to a type or class already named in literature or on the stage, and (3) symbolization of some trait of character or appearance (Mr. Fop, Mr. Glum). Only one of the seven observers consistently invented names that by sound were intended to fit the individual. In this case, however, as in others in both of the experiments, the attempt was frequently made to find a name or a person that was fitting in character as well as in appearance, or even aside from appearance. Observers who are poor visualizers and who at the same time have a 'feeling' for names often tend to typify some trait of character rather than some bodily trait. The two may coincide (fat and good nature); but it is obvious that such coincidence is not necessary.

It seems, then, that the physiognomy of a proper name is at best but one—and that not an important—factor out of the many which determine what personality shall be attached to the name. Authors, no doubt, choose names to 'fit' their characters. We know that Dickens came to *Chuzzlewit* through Sweezleden, Sweezleback, Sweezlewag, Chuzzletoe, Chuzzleboy, Chubblewig, and Chuzzlewig. The name was significant to him; and yet there were various types of Chuzzlewit, as there were various types of Nickleby. Indeed, the applicability of a surname to all the members of a family must, one would suppose, tend to prevent our attaching any special import to the name's physiognomy. As to *Pickwick*, Dickens did not create the name; he took it from a real person. It is worth noting, perhaps, that Seymour's first sketch "was of a long, thin man;" and that the change to the familiar figure came by way of a suggestion of Chapman's, the suggestion "of a friend of mine at Richmond, a fat old beau who would wear drab tights and black gaiters. His name was John Foster."[3] Here is complexity enough! Further, the "short and sonorous" name *Bouvard* is not that of a "slender and active person;" Bouvard is heavy, and Pécuchet is slight; the names —when one knows them—fit excellently, but they fit against Claparède's rule.

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[3] J. Forster. *The Life of Char* [illegible] 2. 88, 91; ii., 1873, 23.

We conclude that the psychological response to unknown proper names is extremely variable. It depends not only upon imaginal type, but also upon associative and attitudinal factors which differ widely in individual observers. At the one extreme, the proper name is merely a word among words; at the other it is, as a proper name, richly suggestive. On the affective side, too, there is wide variation of response, from complete indifference to strongly emotional empathy. Moreover, a highly responsive observer is not uniformly responsive; certain names will 'leave him quite cold.'

We hope to continue this Study (1) by a detailed account of the reactions of certain highly responsive observers, and (2) by a psychological examination of the proper names employed by certain writers of fiction.

A NOTE ON THE COMPENSATION OF ODORS

By E. B. Titchener

In his recent discussion of the compensation of odors, Dr. Henning refers to my work in terms which make a correction necessary.[1]

(1) After remarking that in my *Experimental Psychology* I follow Zwaardemaker—which is quite true—Dr. Henning writes: "ob indessen irgend eine Vp. dabei die Geruchslosigkeit wirklich feststellte, das erfahren wir nicht."

My critic is mistaken. I say that "not every student can get a compensation effect in every experiment;" I emphasize the instability and impermanence of the experimental compensations; I give Zwaardemaker's compensation ratios, and point out that the Cornell results do not always agree with those of Zwaardemaker; and I quote—with the statement "the following are typical laboratory results from two observers"—the records of actual compensations of india rubber by cedarwood and of india rubber by gum benzoin. "In these, and many similar cases," I proceed, "true compensations were found. . . . The nothingness cannot be kept for more than an instant, but it can be refound without difficulty in another trial." Again, after stating that "according to Zwaardemaker all the substances recommended for this experiment are compensatory substances," I add in a footnote: "we can bear out this statement for all the substances but Russian leather."[2] I do not know how I could have shown more plainly that the observers in my laboratory had had experience of olfactory compensation.

(2) Dr. Henning continues: "In seinem Lehrbuch [i., 122] bemerkt er ganz im Gegenteil, dass eine Geruchslosigkeit nicht auftritt, sondern dass zum mindesten der stärkere von zwei Gerüchen wahrgenommen wird, wobei er als Beispiele gerade diejenigen Riechstoffpaare nennt, für die Zwaardemaker das gänzliche Fehlen jedes Geruches behauptete."

I say, on the contrary, that compensation does take place; the paragraph on p. 122 of the *Lehrbuch* follows p. 131 of the *Experimental Psychology*. I add on p. 123 (still following the *Experimental Psychology*): "Der Versuch zeigt, dass zwei Gerüche sich nur selten länger als wenige Sekunden lang kompensieren; es ist leicht, einen ungesättigten Geruch von der Qualität der stärkeren Komponente zu erhalten, aber nicht leicht, eine wirkliche Auslöschung zu erzielen." There is no change of standpoint from the one book to the other.

(3) Dr. Henning mistrusts the olfactometer, and himself finds no trace of complementarism among odors. Whether this conclusion is sound, and our transitory compensations are due to errors of technique, I am not yet prepared to discuss. The object of this Note is to prove that he has misread me, both in English and in German.

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[1] H. Henning, Der Geruch, ii., *Zeits. f. Psychol.*, lxxiv., 1916

[2] *Experimental Psychology,* I., ii., 1901, 133 ff.

In case, however, that other psychologists should care to repeat Zwaardemaker's experiment, I again call attention to the fact that we have not always been able to verify his compensation ratios. Our results, it is true, were obtained in the course of regular laboratory practice; but they are probably as reliable as results from relatively untrained observers can be. I give an illustration. According to Zwaardemaker, 10 cm. india rubber = 5.5 cm. cedarwood. The figures quoted in my *Experimental Psychology* as typical of our own work are 5.5 ± 0.8 and 5.65 ± 0.15 cm. cedarwood. I find in our records, however, a case (March 9, 1900) in which 10 cm. india rubber = 1.44 ± 0.3 cm. cedarwood. Neither experimenter nor observer knew anything of Zwaardemaker's ratio. The observer, who had already worked with india rubber, remarks: "The odor of the india rubber seemed a little weak," and the experimenter adds: "Perhaps on account of the weather, as it was a clear, cold day."

BOOK REVIEW

An Outline of Psychobiology. By KNIGHT DUNLAP. Baltimore, Johns Hopkins Press, 1914, pp. 121.

Dunlap's intention as expressed in his preface was the production of a book for students of psychology (primarily his own) who have had no courses in biology, to convey to them in the limited time at their disposal the elementary information which is absolutely necessary and to stimulate them to further reading.

Dunlap expresses the conviction that psychologists have overestimated the neurological side of biology and have neglected the functional relations of muscle and gland to the nervous system, which he classes as essential requirements for the application of the facts of neurology to psychology.

Considering the object and viewpoint expressed in the preface it comes as a distinct surprise to find that the structural treatment is the dominant one throughout the book and that it is replete with technical terms and with little explanation of a sort which would make it available to those who have had no biology. Another rather striking feature of the treatment is a sort of dogmatism, which makes no mention of the alternative in debated questions, or dismisses them without discussion.

The first chapter deals with the cell and its division from a morphological standpoint, with only two short paragraphs devoted to its chemical and biological aspects.

Chapter two covers in less than six pages (over half of which are illustrations) the whole subject of embryology and the histology of all tissues, except the nervous, muscular and glandular. As might be expected from the space occupied the treatment is too brief to be consistent, either with clarity or adequate consideration. For instance heading No. 6 tells us that the vascular tissue "includes the blood and lymph, the lymph glands and the red marrow of the bones." Apparently the blood and lymph are left to find their way about the body without guiding vessels. The only further discussion of this group is contained in the dependent clause "and develops from the endoderm," thus making the only remark on this great group of structures a positive statement concerning a much debated question.

In the chapter on muscle appears the first proportionate attempt at a functional treatment of the subject. The discussion of the contraction of smooth muscle states that this tissue "cut off from all nervous connection . . . may still contract and relax alternately if subjected to a continuous external stimulus" and overlooks the pregnant fact that the plexuses of Auerbach and Meissner form an intrinsic nervous mechanism which is not ablated in the type of experiment from which these conclusions are drawn.

The chemical treatment of muscular metabolism is too brief to do justice to such a subject. The discussion of fatigue leads us by inference to the conclusion that it is essentially a muscular process. No mention of neuronic fatigue is made. Under the heading "electrical properties of muscle," a brief description is given of the demarcation current and action current and the subject is then discussed in a paragraph which states that they may not be of any special significance and in any case are probably artifactual with no mention of the more recent work on the electrochemical hypothesis of cell excitation and its bearing on the neuromuscular apparatus. The further statement in this last paragraph that there is probably no current in muscle unless electrodes are applied and an external circuit established through them, might also apply to any highly charged electric circuit.

Chapters IV to VII, inclusive, deal with the nervous system in a structural description, in which the emphasis seems more apt to fall on the non-essential than the essential, and which in places is so curtailed that it amounts to little more than a list of structures with only sufficient additional words to form sentences. Possibly no better criticism could be offered than quotation; on page 77, paragraph 3, we read: "On the ventral side of the medulla the olives (olivae), the pyramids (pyramis), and the decussation of the pyramids (decussatio pyramidum), are noticeable. On the dorsal side the cuneate tubercles, the clava, the funiculus gracilis, and the funiculus cuneatus appear. Conspicuous on the floor and side walls of the fourth ventricle (between the stem and the cerebellum) are the striae acusticae (or striae medullares) crossing the area acustica, the eminentia teres (colliculus facialis) and the beginning of the Sylvian aqueduct (aqueductus cerebri)." In contrast with this we read on page 79, paragraph 3: "Above the thalami are the two hemispheres of the cerebrum which are spread out over and behind the thalami and the mid brain. The hemispheres may be considered as ganglia, or groups of ganglia, the cells of which are in the outwardly lying portions (the cortex)." Except for passing mention in an occasional paragraph this is all the description of the cerebrum in the whole book. There is no mention of gyri or sulci nor description of the cortex.

The treatment of the spinal cord is exclusively descriptive, the columns are listed as ascending and descending, but no review of probable function is included and the statements concerning the origin of the different fibre groups are insufficient. For instance—"the fibres in the pyramidal tract are axons of cell bodies in the cerebral cortex. The cell bodies of the fibres in the other descending columns lie in the medulla, pons, cerebellum or midbrain or gray columns of the cord."

Under the visceral division of the nervous system, page 91, paragraph 3, the plexuses of Auerbach and Meissner are mentioned as forming "an independent local system with afferent and efferent fibres having (probably) no communication with the general nerve system" while in the next chapter, page 101, paragraph 1, "these plexuses contain numerous ganglion cells and are possibly connected with fibres from the other parts of the autonomic system." The discrepancy here is striking but even more so is the obvious failure to appreciate the functional significance of this system on control of involuntary muscular contraction (*vide supra*). No mention is made of the structural characteristics of these intrinsic nervous plexuses nor of the interesting phylogenetic correlation with that of Medusa.

Chapter VIII deals with the glands of the body and the material is handled much as in previous chapters. For instance the inclusion of the coccygeal and carotid bodies which with the adrenal medulla form part of the chromafine system among the principal members of the endocrine glands and the exclusion of the testes and ovaries (except for mention in a foot note) certainly do not conform with the accepted opinion concerning the relative importance of these structures.

The final chapter which deals with the functional interrelation of receptors, neurons and effectors is chiefly remarkable for what it omits. The paragraphs on cerebral localization take an attitude so adverse to the great mass of evidence from anatomical, pathological and experimental studies as to be inconceivable did we not bear in mind that we are dealing with a book in which there appears a brain without convolutions in a body without blood vessels.

Dunlap is to be congratulated on his intentions as expressed in his preface but the subordination of the functional to the structural, the unhappy choice of material for emphasis, the dogmatic form, and the errors and omissions, would seem to warrant the opinion that the book has failed to accomplish its task and serve to question gravely its value.

Pennsylvania Hospital for the Insane. S. T. Orton.

BOOK NOTES

Das Denken und die Phantasie; psychologische Untersuchungen nebst Exkursen zur Psychopathologie, Aesthetik und Erkenntnistheorie. Von RICHARD MÜLLER-FREIENFELS. Leipzig, Barth, 1916. 341 p.

This is a notable work, which seeks to show that thought and fantasy are not reproductive but reactive phenomena. Starting with the idea of *Vorstellung,* the author urges that not *Anschaulichkeit* but a subjective consciousness of direction, which is best described as *Einstellung,* is the essential thing, and that this cannot be referred back to sensations. So perception can be understood not as made of reproduced factors but as essentially made of affective motor "*Stellungnahmen;*" also, judgment and idea, instead of being regarded as concepts or their transformations or compositions, must be considered as centers of action, that is, as activities or *Stellungnahmen.* On the basis of these fundamental ideas, the author seeks to develop new points of view for the higher thought processes. The old laws of association are more or less illuminated by this view, but the teleological motive in thought and fantasy comes into the foreground, so that the thinker is an artistic creator. Thus the teleological elements in thought are dominant. This opens a perspective into psychopathology, aesthetics, and a theory of knowledge. It affects, of course, the relation of the psyche to the outer world, and the relations of psychic phenomena to each other, especially to sensations and their central elements. Since these elements are not sensations or their transformations but attitudes in reaction to these elements we have established a new point of view.

The mental life of monkeys and apes: a study of ideational behavior. By ROBERT M. YERKES. (Behavior Monographs, Vol. 3, No. 1, 1916.) Cambridge, Henry Holt & Co., 1916. 145 p.

This is not only the latest but by far the most important study that has yet been made in its field. Professor Yerkes spent a year in Southern California under the most favorable conditions, and probably succeeded in getting nearer to the soul of these creatures than anyone else has ever done. It is impossible to do justice to this work without more space than is at our disposal.

The photoplay; a psychological study. By HUGO MÜNSTERBERG. New York, Appleton & Co., 1916. 233 p.

This is another popular book by this fecund, popular author, a list of whose publications almost fills a page. Here he dips, with the assumed authority of a psychologist, into the moving picture, discussing depth of movement, attention, memory and imagination, emotions, but in a kind of perfunctory and remote way. The second part is entitled "The Esthetics of the Photoplay," and here he brings forward well known ideas on the purpose of art, the means of the various arts, the demands and the function of the photoplay. To the thought of the present reviewer this book is somewhat too journalistic.

The psychology of the common branches. By FRANK NUGENT FREEMAN. Boston, Houghton Mifflin Co. (c. 1916). 275 p.

This work attempts to apply the knowledge which we have recently accumulated in psychology to the concrete problem of instruction in the elementary school. The analysis of the learning processes, which began with Huey's psychology of reading, was somewhat of an epoch. The present book discusses handwriting, drawing, reading, music, spelling, history, geography, mathematics, and natural sciences, from this point of view.

The greater tragedy, and other things. By BENJAMIN APTHORP GOULD. New York, G. P. Putnam's Sons, 1916. 189 p.

The author believes that the year 1916 is the most important year in the history of the United States, because it now achieves its majority. The choice the war puts up to us is service or sloth. Shall we be workers or drones? The twenty-one papers printed in this book are stimulating reading. Some of the best are entitled "The Lusitania," "The War Against War," "Sir Edward Grey," "Our National Conscience," "Canadian Friendship," "Isolation."

Nervous children; prevention and management. By BEVERLEY R. TUCKER. Boston, Richard G. Badger (c. 1916). 147 p.

This book is a collection of papers, most of which had been published elsewhere. The chapters are on the child's nervous system, heredity and environment, nervous and mental development and personality, habit, eugenics and sex hygiene, cause and prevention of nervousness, training of nervous children, defective and feebleminded children, puberty and adolescence.

Manual of vital function testing methods and their interpretation. By WILFRED M. BARTON. Boston, Richard G. Badger (c. 1916). 255 p.

The writer tells us that the information this book contains is scattered and much of it has never been brought together before. He wants to test the vital functions, such as liver, kidneys, heart, pancreas, ductless glands. For the heart, for instance, he describes eight different tests, their value and limitations, and the technical methods of computing the applications of each test.

Fatigue study; the elimination of humanity's greatest unnecessary waste. By FRANK B. GILBRETH and LILLIAN M. GILBRETH. New York, Sturgis & Walton, 1916. 159 p.

"In the final analysis, that organization is best that has the best quality of workers." The important chapters here are, "A Fatigue Survey," "Home Reading Box Movement," "Preliminary Fatigue Elimination," "Fatigue Museum," "Fatigue Measurement," "Making Adjustments," "The Future."

Making Life a masterpiece. By ORISON SWETT MARDEN. New York, T. Y. Crowell Co. (c. 1916). 329 p.

These are wholesome and stimulating talks about practical dreamers, where your opportunity is, the triumph of common virtues, physical vigor, curing the curse of indecision, unlocking your possibilities, bettering our best, the will to succeed, etc.

Provision for the study of monkeys and apes. By ROBERT M. YERKES. (Reprinted from Science, N. S., Vol. XLIII, No. 1103, p. 231-234, Feb. 18, 1916.)

Qualitative differences between levels of intelligence in feeble-minded children. By LOUISE ELLISON ORDAHL and GEORGE ORDAHL. Monograph Supplement, Journal of Psycho-Asthenics, Vol. 1, No. 2, June, 1915. 50 p.

Review of Meumann on tests of endowment. By LEWIS M. TERMAN. (Reprinted from Journal of Psycho-Asthenics, Vol. XIX, No. 2, December, 1914.) p. 75-199.

The Johns Hopkins Hospital reports. Volume XVII. Baltimore, Johns Hopkins press, 1916. v. p.

Report of the neurological department of the Cincinnati Hospital from the date of its establishment, 1894, to 1914, inclusive, a period of twenty-one years. By ROBERT INGRAM. (Reprinted from The Lancet-Clinic, November 20, 1915.) 10 p.

The psychology and physiology of mirror-writing. By JUSTIN K. FULLER. (University of California Publications in Psychology, Vol. 2, No. 3, p. 199-265, May 8, 1916.) Berkeley, University of California Press.

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THE AMERICAN
JOURNAL OF PSYCHOLOGY
Founded by G. Stanley Hall in 1887

Vol. XXVII OCTOBER, 1916 No. 4

THE MEASUREMENT OF ATTENTION IN THE FIELD OF CUTANEOUS SENSATION[1]

By K. M. Dallenbach, Cornell University

We have recently published a study in the measurement of attention within the field of audition.[2] Since that time we have repeated the work within the field of cutaneous sensation, and it is our purpose in this paper to present these additional and confirmatory results. We do not intend at this time to criticise the methods of dealing with the problem of attention, or to draw theoretical conclusions from our results. At some later date we hope to deal comprehensively with the facts, the methods, and the criticisms of the measurement of attention; but we have wished not to delay unduly the publication of these results.

Methodically, the present study differs in no important respect from its predecessor, except that cutaneous sensation is substituted for auditory, and that the attributive changes are therefore changes of intensity and extent instead of changes of intensity and pitch. For general procedure and details of method the reader is referred to the earlier paper. In general our tables can be understood from the description there given, if substitution of extent for pitch be made. It would be well, then, if the present paper were read with the other at hand.

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[1] Presented at the Chicago meeting of the American Psychological Association, December, 1915. The experimental work was done in the psychological laboratory of the University of Oregon in 1913-15.

[2] K. M. Dallenbach, this Journal, XXI [illegible] R. Geissler, *ibid.* XX, 1909, 502-529, first used [illegible]

Observers.—The observers were Mr. E. Good (G), Miss G. Cross (C), and Mr. M. Goodwin (Gn). All three were advanced students in psychology, and had had considerable practice and experience in introspection. They all worked without knowledge of the problem, and they served regularly three hours a week.

METHODS

Preliminary Training in Introspection.—The procedure was practically the same as in the earlier work,[3] except that the interval was shortened (30 to 60 secs.).

Practice was continued for four months. At the end of the third month the scale of degrees of clearness[4] had been worked out by all observers.

Preliminary Practice in Observation under the Normal Conditions of the Main Experiments.—Our stimulus was a faradic current from a Harvard inductorium. Four large Edison-Lalande cells (E. M. F. 0.85), arranged in series, were used to actuate the primary coil. The strength of the primary current was practically constant, for at the beginning of every experimental hour we reduced the current, by the aid of a small rheostat, to 25 amperes. Measurement was made by a small ammeter shunted into the circuit. There was a slight reduction in the current as the hour and the experimental work progressed; but since only 4-6 experiments were conducted during the hour, and since an experiment lasted only 20 seconds, the reduction was very slight and the current might be regarded as constant. In any case, as Martin[5] has shown, a slight variation in the strength of the primary current will cause no appreciable change in the strength of the secondary.

The intensity of the secondary current was controlled by a shift in the position of the secondary coil. The extensity of the stimulation was controlled by a three-way switch which permitted us to shunt the current to any one of three electrodes which served as stimulus.

The common electrode was placed at the back of the neck over the *vertebra prominens* and the other, the stimulus (three concentric copper rings) upon the dorsal side of the left forearm about 10 cm. below the elbow joint. The areas stimulated by the three electrodes were all supraliminally different. This was determined empirically and separately for each observer, and a value was taken which was well above the limens as follows: the area of the smallest electrode was 7.06 sq. mm.; that of the medium, 94.25 sq. mm.; and that of the largest, 188.69 sq. mm. The smallest electrode was round, 3 mm. in diameter; the medium was a ring 2 mm. in width, with an inside diameter of 13 mm.; the largest was likewise a ring 2 mm. in width, with an inside diameter of 28 mm. The medium was insulated from the other two by sealing wax. The cutaneous sensations were localized at the arm. At high intensities the current was also perceived at the neck: a limit which, once determined, we never approached. Owing to the difference in areas stimulated, and consequently in the amount of resistance offered to the current, the sensations from the three electrodes were not of the same intensity. Therefore resistance was added to these circuits such that the intensity of the cutaneous sensations was equal for equal intensities of the current. The amount of resistance to be added was different for every observer, and individual rheostats were accordingly provided.[6]

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[3] Pp. 467f. [4] P. 468.

[5] E. G. Martin, *Am. J. of Physiology*, XXII, 1908, 61-74; 116-132.

The changes in the intensity of the cutaneous sensations were also supraliminally different, extending in every case through two just noticeable differences. These steps were computed separately for every observer by the method of Just Noticeable Differences.

The three-way switch, by which the current was shifted to the different arm-poles, and the secondary coil of the inductorium were fitted with large scales and long moving arms, which permitted us to make gross movements in their adjustment.

The observer's left arm and hand, dorsal side up, were secured in a plaster cast which prohibited all movement, whether voluntary or involuntary.[7] The electrode was held in position, 10 cm. below the elbow joint, by an elastic band, which insured not only that the electrode would be returned to the same area from experiment to experiment, but also that it would be held on the arm at a constant pressure.

The pressure, including the weight of the electrode, was 200 grams for G; 196 for C; and 199 for Gn. The area stimulated was frequently shaved, and before every experiment was bathed in a concentrated salt solution.

The electrode at the *vertebra prominens* was composed of a nickel-plated brass plate 3 cm. by 6 cm. It was covered by a felt pad 1 cm. in thickness. This pad was thoroughly saturated at the beginning of every hour with a concentrated solution of salt. This electrode was secured in position by an elastic tape encircling the neck.

The observer was seated at a table in a dark room with his left arm secured in the manner just mentioned, and his right hand resting upon a silent electrical key; and the experimenter was placed with the induction coil and the control apparatus in a room near by. The observer was, moreover, enclosed in a muslin booth, illuminated from above by an electric light controlled from the experimenter's desk. The experiments were conducted in dark-adaptation, and the light was turned on at the end of an experiment so that the observer could write his introspection and bathe his arm, while the experimenter was preparing the apparatus for the next experiment.

The graphic records of times were obtained in the same manner as in the earlier experiment.[8]

The *instructions* were those of the previous experiment, adapted for the altered conditions.[9]

The time of an experiment was decreased to 20 secs. in order to increase the accuracy of report.

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[6] My thanks are due to Prof. Wm. P. Boynton, of the department of physics at the University of Oregon, who aided me in standardizing my apparatus, and who suggested the lead-pencil rheostats that were used to equalize the intensity of the cutaneous sensations; and to Mr. Wm. H. Kiler, of the engineering department of the Southern Pacific Railroad, who aided me in constructing and setting up the apparatus.

[7] We at first attempted to work with the volar side of the arm, but soon abandoned it for the dorsal side. The change was imperative because of the great number of motor centers on the volar side. We found that it was impossible to stimulate any area upon this side of the arm, within the necessary limits of our experiment, without causing some muscular contraction. This distraction was eliminated by shifting the electrode to the other surface.

[8] P. 469. [9] P. 469.

The detailed *arrangement of series* followed the general principles laid down in our first paper.[10] There were 76 series in all. These involved 104 changes of extensity and 104 changes of intensity. Half of each were changes in the direction of greater, and half in the direction of smaller.

In this practice work G performed 40 experiments; C, 68; and Gn, 58. The work covered a period of three months: April, May, part of October, and part of November, 1914.

Single Task Method.

Six distractors were employed:

1. Flicker (8 rhythms and 10 intensities).
2. Buzzer (3 intensities).
3. Electric bell (3 intensities).
4. Metronome (4 rates: 60, 90, 120, and 150).
5. Flicker and metronome (with above variations).
6. Phonograph.

The flicker was produced by means of the electric bulb which was used to light the booth in which the observer worked. The illuminating current was made and broken very rapidly by a device connected with a small motor. Ten variations of intensity from very weak to the full intensity of the light were given by a rheostat. Eight cadences were used, which varied from regular, slow, and rapid, to discontinuous and jerky.[11]

Different intensities of buzzer and bell were obtained by varying the strength of the current.

The phonograph stood upon the experimenter's desk; the sound was conveyed to the observer by a speaking tube.

Instructions and *series* were practically identical with those for the Single Task Method in the earlier experiment,[12] except for the changed conditions and the reduction of the period to 20 secs.

Every observer performed 76 experiments under this method, all during the winter of 1915.

As in our other experiments,[13] only the higher degrees of attention occurred. Hence it was necessary to supplement the results by those of the Double Task Method.

Double Task Method.

The second task was that of continuous addition or subtraction. The procedure was in general that of the earlier paper.[14] Three 'difficulties' were used, and the numbers were so chosen as to give greater uniformity within each 'difficulty' and a greater difference between them. The numbers were presented auditorily by an Edison dictaphone connected with the speaking tube which led to the observer's booth.

The *instruction* was essentially that of the first experiment, except that the questionary was omitted.[15]

The experiments were made in the last part of May and first part of June, 1915. Every observer performed 60 experiments: a reduction from the complete series of 76 necessitated by the ending of the term.

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[10] *Cf.* pp. 470f.

[11] The method proved less satisfactory than that with the episcotister, *op. cit.* 472f., but was necessitated by the lack of a silent motor.

[12] P. 474. [13] P. 490. [14] P. 491. [15] P. 492.

RESULTS

Table I shows for the Single Task Method the degrees of clearness given in the various reports in relation to the accuracy of the report and the kind and direction of change. It is in every way analogous to Table I of our earlier paper.[16]

Table II shows the same results for the Double Task Method. It may be understood by reference to Table X of the first paper.[17]

Tables I and II give only the number of cases occurring under their rubrics. A comparison of the data is more clearly afforded by Tables III and IV. These tables are the analogues of Tables II[18] and XI[19] in the first paper.

The tables bear out in every detail the findings of our first study. The higher degrees of clearness favor right cases and subjective reports. The Double Task Method brings a greater range of degrees of clearness.

The weighted summaries of Tables V and VI are obtained from the summaries of Tables III and IV in accordance with the method of weighting laid down in our previous study.[20] Tables V and VI are analogous to Tables III[21] and XII[22] of that study. As in our former work, we see that the crests of the curves of the right judgments lie, in the case of either method, above the crests of the curves of the wrong judgments. We have thus reestablished the relation between introspectively distinguished variations of clearness and accuracy of work performed.

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[16] Pp. 474ff.
[17] Pp. 494ff.
[18] Pp. 478ff.
[19] Pp. 496ff.
[20] P. 481.
[21] P. 481.
[22] P. 499.

TABLE I

(Single Task Method)

NUMBER OF REPORTS, CLEARNESS OF MENTAL PROCESSES AT TIME OF CHANGE, ACCURACY OF REPORT, AND KIND AND DIRECTION OF CHANGE

Report	Kind	Direction	O.	RIGHT							WRONG						
				10–9	9–8	8–7	7–6	6–5	5–4	4–3	10–9	9–8	7–8	7–6	6–5	5–4	4–3
KIND AND DIRECTION	INTENSITY	weaker	G	14	16	6	2						2				
			C	11	9	3	1							1			
			Gn	15	13	4	1	1									
		stronger	G	21	8	4	4							1		1	
			C	21	4	1	1	1	2			1		1			
			Gn	20	14	5	1	4			1	1		1			
	EXTENSITY	smaller	G	19	8	3		1					2	1	1	1	
			C	23	2	2							2	3	2	2	
			Gn	5	16	4	3				3		3	1	2	2	
		larger	G	17	10	5	2						1	1			
			C	24	4							1			4	2	
			Gn	11	7	4	1	3	1			1	1	2			
KIND	INTENSITY	weaker	G		1	1			1		1	1	2	1			
			C	1	3	1	2		3			2		1	1	1	
			Gn			1	3								1		
		stronger	G	1			1		1		1	1	3	2			
			C	1	1	2	3						1	1	1	1	
			Gn			2					1	1					
	EXTENSITY	smaller	G	1	2	1	1	1			2	3		2			
			C	2	1	1						1		1	1		
			Gn	1				1				4		2		2	
		larger	G		2							1	4	4			
			C			1	2	2					3		1		
			Gn								3	11		2			
DIRECTION	INTENSITY	weaker	G	1	1	2	1					1	1			1	
			C		2		1	1	1		1	3	1	2		3	
			Gn					1					1	3			
		stronger	G	1	1	3	2				1			1		1	
			C			1	1	1	1		1	1	2	3			
			Gn	1	1								2				
	EXTENSITY	smaller	G	2	3		2				1	2	1	1	1		
			C		1		1	1			2	1	1				
			Gn		4		2		2		1				1		
		larger	G		1	4	4					2					
			C			3		1					1	2	2		
			Gn	3	11		2										

TABLE I—(*Continued*)

Kind	Direction	O.	SUBJECTIVE							NO-REACTION						
			10–9	9–8	8–7	7–6	6–5	5–4	4–3	10–9	9–8	8–7	7–6	6–5	5–4	4–3
INTENSITY	weaker	G											1	1	1	
		C	2									1	2	2	8	
		Gn		1		1						4	3	3	3	
	stronger	G	2											1		
		C		1						1	1	1	1	1	3	
		Gn											1			
EXTENSITY	smaller	G		1										1		
		C		1							1	2	1		4	
		Gn										1	1		1	
	larger	G													1	
		C	2											2	7	
		Gn		1								2	3			

TABLE II

(Double Task Method)

NUMBER OF REPORTS, CLEARNESS OF MENTAL PROCESSES AT TIME OF CHANGE, ACCURACY OF REPORT, AND KIND AND DIRECTION OF CHANGE

Report	Kind	Direction	O.	RIGHT									WRONG								
				10-9	9-8	8-7	7-6	6-5	5-4	4-3	3-2	2-0	10-9	9-8	8-7	7-6	6-5	5-4	4-3	3-2	2-0
KIND AND DIRECTION	INTENSITY	weaker	G				2	1											5	7	
			C	1		1	1	1										1	1		
			Gn			1	1		1												
		stronger	G			1	1												3	4	
			C		2					1						1				1	
			Gn			2	4	3	3	1	1								5	4	
	EXTENSITY	smaller	G	1	1	2	1							1		1	2		4	3	
			C	5			1									1				1	
			Gn							1	1								7	2	1
		larger	G	1		1				1	1					2		1	5	4	
			C	2		1	1								1	1	1	1		2	1
			Gn			2											1		1	2	
KIND	INTENSITY	weaker	G					1	2	1	1						1				
			C		1			1		1				1	1	1					
			Gn				1														
		stronger	G				1	1	1	1	1							1			
			C			1								3	3	2	1				
			Gn					1		1	2										
	EXTENSITY	smaller	G		1		1			2							2				
			C	1										1		1					
			Gn				1									1	1		1		
		larger	G		1		1				1			1		2					
			C											2		2					
			Gn					1			1					3	2		1		
DIRECTION	INTENSITY	weaker	G					1									1	2	1	1	
			C		1	1	1							1			1		1		
			Gn													1					
		stronger	G						1							1	1	1	1	1	
			C		3	3	2	1							1						
			Gn														1		1	2	
	EXTENSITY	smaller	G					2						1		1			2		
			C		1		1						1								
			Gn				1	1		1						1					
		larger	G		1		2							1		1				1	
			C		2		2														
			Gn				3	2		1							1			1	

TABLE II—(*Continued*)

Kind	Direction	O.	SUBJECTIVE									NO-REACTION								
			10-9	9-8	8-7	7-6	6-5	5-4	4-3	3-2	2-0	10-9	9-8	8-7	7-6	6-5	5-4	4-3	3-2	2-0
INTENSITY	weaker	G															2		4	11
		C	1													2	1		5	18
		Gn																3	10	18
	stronger	G														2	1	3	4	9
		C			1											1	2	2	2	13
		Gn																	5	5
EXTENSITY	smaller	G																1	5	6
		C														1	1	4	5	10
		Gn																	12	9
	larger	G															1	2	6	7
		C													2		1	3	3	15
		Gn															1		10	10

TABLE III

(Single Task Method)

NUMBER OF CASES GROUPED ACCORDING TO THE KIND AND DIRECTION OF THE OBJECTIVE CHANGE; THE ACCURACY OF THE OBSERVER'S REPORTS; AND THE CLEARNESS OF THE MENTAL PROCESSES AT THE TIME OF CHANGE; WITH SUMMARY.

Kind or Direction	Report	O.	CLEARNESS						
			10–9	9–8	8–7	7–6	6–5	5–4	4–3
INTENSITY	Right	G	36	25	11	7		2	
		C	34	17	7	7	1	5	
		Gn	35	27	12	5	5		
	Wrong	G	2	2	7	4		1	
		C		3	1	4	2	2	
		Gn	2	2		1	1		
	Subjective	G	2						
		C	2	1					
		Gn		1		1			
	No reaction	G				1	2	1	
		C	1	1	2	3	3	11	
		Gn			4	4	3	3	
EXTENSITY	Right	G	37	22	9	3	2		
		C	49	7	4	2	2		
		Gn	17	23	8	4	4	1	
	Wrong	G	2	4	7	8	1	1	
		C		2	5	4	8	4	
		Gn	6	16	4	7	2	4	
	Subjective	G		1					
		C	2	1					
		Gn		1					
	No reaction	G					1	1	
		C		1	2	1	2	11	
		Gn			3	4		1	
WEAKER OR SMALLER	Right	G	36	28	11	5	1		
		C	34	14	5	3	2	1	
		Gn	20	33	8	6	2	2	
	Wrong	G	1	3	6	2	2	2	
		C	3	4	4	6	2	5	
		Gn	4		4	4	3	2	
	Subjective	G		1					
		C	2	1					
		Gn		1		1			
	No reaction	G				1	2	1	
		C		1	3	3	2	12	
		Gn			5	4	3	4	
LARGER OR STRONGER	Right	G	39	20	16	12			
		C	45	8	5	2	3	3	
		Gn	35	33	9	4	7	1	
	Wrong	G	1	2	1	3		2	
		C	1	3	3	6	6	2	
		Gn	1	2	3	3			
	Subjective	G	2						
		C	2	1					
		Gn		1					
	No reaction	G					1	1	
		C	1	1	1	1	3	10	
		Gn			2	4			

TABLE III—(*Continued*)

Kind or Direction	Report	O.	Clearness						
			10–9	9–8	8–7	7–6	6–5	5–4	4–3
SUMMARY	Right	G	148	95	47	27	3	2	
		C	162	46	21	14	8	9	
		Gn	107	116	37	19	18	4	
	Wrong	G	6	11	21	17	3	6	
		C	4	12	13	20	18	13	
		Gn	13	20	11	15	6	6	
	Subjective	G	4	2					
		C	8	4					
		Gn		4		2			
	No reaction	G				2	6	4	
		C	2	4	8	8	10	44	
		Gn			14	16	6	8	

TABLE IV

(Double Task Method)

NUMBER OF CASES GROUPED ACCORDING TO THE KIND AND DIRECTION OF THE OBJECTIVE CHANGE; THE ACCURACY OF THE OBSERVER'S REPORTS; AND THE CLEARNESS AT TIME OF CHANGE; WITH SUMMARY.

Kind and Direction	Report	O.	Clearness								
			10–9	9–8	8–7	7–6	6–5	5–4	4–3	3–2	2–0
INTENSITY	Right	G			1	4	3	3	2	2	
		C	1	3	2	1	2		2		
		Gn			3	6	4	4	2	3	
	Wrong	G					1	1	8	11	
		C		4	4	4	1	1	1	1	
		Gn							5	4	
	Subjective	G									
		C	1		1						
		Gn									
	No reaction	G					2	3	3	8	20
		C					3	3	2	7	31
		Gn							3	15	23
EXTENSITY	Right	G	2	3	3	3			3	2	
		C	8		1	2					
		Gn			2	1	1		1	2	
	Wrong	G		2		5	4	1	9	7	
		C		3	1	5	1	1		3	1
		Gn				4	4		10	4	1
	Subjective	G									
		C									
		Gn									
	No reaction	G						1	3	11	13
		C							7	8	25
		Gn								22	19

TABLE IV—(*Continued*)

Kind and Direction	Report	O.	Clearness								
			10–9	9–8	8–7	7–6	6–5	5–4	4–3	3–2	2–0
SMALLER OR WEAKER	Right........	G	1	1	2	3	4				
		C	6	2	2	4	1				
		Gn			1	2	1	1	2	1	
	Wrong........	G		2		2	3	2	12	11	
		C	1	1		1	1	1	2	1	
		Gn				2			7	2	1
	Subjective....	G									
		C	1								
		Gn									
	No reaction...	G						2	1	9	17
		C					3	2	4	10	28
		Gn							3	22	27
LARGER OR STRONGER	Right........	G	1	1	2	3		1	1	1	
		C	2	7	4	5	1		1		
		Gn			4	7	5	3	2	1	
	Wrong........	G		1		4	1	2	9	10	
		C			2	2	1	1		3	1
		Gn					3		7	9	
	Subjective....	G									
		C			1						
		Gn									
	No reaction...	G					2	2	5	10	16
		C				2	1	3	5	5	28
		Gn						1		15	15
SUMMARY	Right........	G	4	5	8	13	7	4	6	5	
		C	17	12	9	12	4		3		
		Gn			10	16	11	8	7	7	
	Wrong........	G		5		11	9	6	38	39	
		C	1	8	7	12	4	4	3	8	2
		Gn				6	7		29	19	2
	Subjective....	G									
		C	2		2						
		Gn									
	No reaction...	G					4	8	12	38	66
		C				4	8	10	18	30	112
		Gn						2	6	74	84

TABLE V

(Single Task Method)

WEIGHTED SUMMARIES

O.	Reports	Clearness					
		100–90	90–80	80–70	70–60	60–50	50–40
G	Total right..................	148.0	95.0	47.0	27.0	3.0	2.0
	Total wrong..................	10.0	13.0	21.0	19.5	10.5	11.0
C	Total right..................	162.0	46.0	21.0	14.0	8.0	9.0
	Total wrong..................	14.5	21.0	23.0	30.0	30.5	68.0
Gn	Total right..................	107.0	116.0	37.0	17.0	18.0	4.0
	Total wrong..................	13.0	24.0	28.5	37.0	13.5	16.0

TABLE VI

(Double Task Method)

WEIGHTED SUMMARIES

O.	Reports	CLEARNESS								
		100-90	90-80	80-70	70-60	60-50	50-40	40-30	30-20	20-00
G	Total right......	4.0	5.0	8.0	13.0	7.0	4.0	6.0	5.0	
	Total wrong....		5.0		16.0	15.0	22.0	53.0	86.5	82.5
C	Total right......	17.0	12.0	9.0	12.0	4.0		3.0		
	Total wrong....	3.0	8.0	9.0	17.0	14.5	16.5	25.5	45.5	142.5
Gn	Total right......			10.0	16.0	11.0	8.0	7.0	7.0	
	Total wrong....				6.0	7.0	2.5	36.5	111.5	107.0

SPECIAL POINTS

(1) The *relation between the observers' reports and the kind and direction of objective change* is shown in Tables VII and VIII, which correspond respectively to Tables IV[23] and XIII[24] of the first study. Table VII shows for all observers that more intensive than extensive changes are correct, that intensive changes are more attention-compelling than extensive, and that changes to a larger and stronger cutaneous stimulus are more compelling than changes to a smaller and weaker. Table VIII does not show a uniform tendency; the work in the Double Task Method was probably not influenced by anything else than fluctuation of attention.

(2) The *promptness of voluntary action* as a measure of attention[25] is shown, irrespectively of the correctness of the judgments, in Tables IX and X (analogous to Tables V[26] and XIV[27] of the first paper).

TABLE VII

(Single Task Method)

RELATION BETWEEN THE OBSERVER'S REPORTS AND THE KIND AND DIRECTION OF THE OBJECTIVE CHANGES

O.	KIND								DIRECTION							
	INTENSITY				EXTENSITY				DOWN				UP			
	R.	W.	S.	N.	R.	W.	S.	N.	R.	W.	S.	N.	R.	W.	S.	N.
G	81	16	2	4	73	23	1	2	81	16	1	4	87	9	2	2
C	71	12	3	21	64	23	3	17	59	24	3	21	66	21	3	17
Gn	84	6	2	14	57	39	1	8	71	17	2	16	89	9	1	6
Total	236	34	7	39	194	85	5	27	211	57	6	41	242	39	6	25

Down, smaller or weaker. Up, larger or stronger. R., right. W., wrong. S., subjective. N., no-reaction.

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[23] P. 482. [24] P. 501.

[25] *Cf.* H. Woodrow, *Psychol. Rev. Monog. Suppl.*, XVII, no. 5, who asserts "that reaction time may vary without corresponding variation in attention" (p. 139), yet admits that "the absolute increase in reaction time produced by the use of unfavorable intervals as a distractor varies inversely as the degree of attention" (pp. 12, 98).

[26] P. 484. [27] P. 501.

TABLE VIII

(Double Task Method)

RELATION BETWEEN THE OBSERVER'S REPORTS AND THE KIND AND DIRECTION OF THE OBJECTIVE CHANGES

O.	KIND								DIRECTION							
	INTENSITY				EXTENSITY				DOWN				UP			
	R.	W.	S.	N.	R.	W.	S.	N.	R.	W.	S.	N.	R.	W.	S.	N.
G	15	21	..	36	16	28	..	28	11	32	..	29	10	27	..	35
C	11	16	2	46	11	15	..	45	15	8	1	47	20	10	1	44
Gn	22	9	..	41	7	23	..	42	8	12	..	52	22	19	..	31
Total	48	46	2	123	34	66	..	115	34	52	1	128	52	56	1	110

Down, smaller or weaker. Up, larger or stronger. R., right. W., wrong. S., subjective. N., no-reaction.

TABLE IX

(Single Task Method)

AVERAGE REACTION-TIME OF THE OBSERVER'S REPORT IN SECONDS AT THE DIFFERENT LEVELS OF ATTENTION

O.		CLEARNESS								
		100–90	90–80	80–70	70–60	60–50	50–40	40–30	30–20	20–00
G	Av. R.	1.19	1.18	1.34	1.55	1.93				
	m. v. . .	.34	.32	.35	.45	.43				
	No. . . .	73	49	23	21	3				
C	Av. R.	1.20	1.47	1.66	1.51	1.80	2.30			
	m. v. . .	.38	.45	.52	.50	.73	.82			
	No. . . .	91	13	16	13	7	9			
Gn	Av. R.	1.04	.94	1.20	1.25	1.09	1.29			
	m. v. . .	.32	.28	.51	.44	.20	.25			
	No. . . .	41	71	14	12	10	7			

Av. R. average reaction time. m. v., mean variation. No., number of cases.

TABLE X

(Double Task Method)

AVERAGE REACTION-TIME OF THE OBSERVER'S REPORT IN SECONDS, AT THE DIFFERENT LEVELS OF ATTENTION

O.		CLEARNESS								
		100–90	90–80	80–70	70–60	60–50	50–40	40–30	30–20	20–00
G	Av. R.	0.70	0.95	0.60	0.79	0.95	0.87	1.68	1.50	
	m. v. . .	.30	.40	.20	.38	.31	.27	.92	.75	
	No. . . .	2	6	4	13	6	7	18	17	
C	Av. R.	1.32	1.30	1.42	1.80	1.47		.80	2.57	1.90
	m. v. . .	.66	.40	.78	.82	.96		.00	1.37	.00
	No. . . .	10	5	8	7	4		2	4	1
Gn	Av. R.			1.02	0.98	0.97	1.17	1.18	0.99	1.40
	m. v. . .			.44	.30	.19	.41	.37	.11	.00
	Ne. . . .			5	12	8	4	15	12	1

Av. R., average reaction time. m. v., mean variation. No., number of cases.

The coefficients of correlation (figured by Pearson's 'product moments' method) and the probable error are, for the Single Task Method:

O.	Correlation	P.E.
G	—0.98	0.011
C	—0.64	0.162
Gn	—0.70	0.140

and for the Double Task Method:

O.	Correlation[28]	P.E.
G	—0.74	0.101
C	—0.91	0.047
Gn	—0.33	0.247

These results corroborate those which we obtained in our first paper, and lead us to the conclusion that reactions of this sort are reliable indices of attention.

(3) In our earlier study we pointed out that the *mean variation* has frequently been used as a measure of attention, and our results at that time tended to confirm such a view. The results of the present study, however, are not conclusive. The correlation between the mean variations and the degrees of clearness, the data of which appear in Tables IX and X, is (Pearson's method) for the Single Task Method:

O.	Correlation	P.E.
G	—0.85	0.063
C	—0.94	0.023
Gn	+0.30	0.239

and for the Double Task Method:

O.	Correlation[29]	P.E.
G	—0.65	0.115
C	—0.83	0.064
Gn	+0.29	0.242

The figures for G and C corroborate our previous findings,[30] whereas Gn's results are contradictory. The difference may be due to the different interpretation that Gn placed upon his instructions, and also to the fact that he frequently reported change without being able to tell the kind or direction.[31] As the delayed reactions show, he did not interpret the directions to mean "react as quickly as possible", but merely "react in order to report." The reactions might therefore be made at his convenience. It is possible that, in the changes which were clear, he sometimes delayed his replies until he was absolutely certain both as to Kind and as to Direction, while at other times he reacted as soon as the change was perceived (this would account for the small reaction times and the large mean variation in the high degrees of clearness); and that, in the changes which were obscure, he always reacted as soon as he became aware of the changes, because, no matter how long he delayed, they were too obscure for him to tell kind or direction (this would account for the long reaction times and the small variations of the low degrees of clearness). We cannot offer more than this general and hypothetical explanation of the discrepancy.

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[28] In the computation of these correlations the data under the 7th and 9th rubrics for C, and the 9th rubric for Gn, were omitted because they represented too few cases: 2, 1, and 1 cases respectively.

[29] See preceding foot-note.

[30] Pp. 485, 502.

[31] Such reports were counted as wrong as to both Kind and Direction, and two points were added to the wrong replies in the weighted results. A half point was thereby credited to the observer; for, had he not noted the fact of change, 2.5 points would have been added to the side of 'wrong.'

(4) The *relation of the reaction times to the Kind and Direction of the objective change* appears for the two methods in Tables XI and XII, which correspond to Tables VI[32] and XV[33] of the first study.

TABLE XI

(Single Task Method)

RELATION BETWEEN THE OBSERVER'S REACTION TIME IN SECONDS AND THE KIND AND DIRECTION OF THE OBJECTIVE CHANGE

O.	KIND						DIRECTION					
	INTENSITY			EXTENSITY			DOWN			UP		
	Av.	m.v.	No.	Av.	m.v.	No.	Av.	m.v.	No.	Av.	m.v.	No.
G	1.28	.40	82	1.25	.32	87	1.29	.38	82	1.24	.39	87
C	1.54	.55	73	1.27	.43	76	1.42	.48	76	1.39	.52	73
Gn	1.06	.38	71	1.02	.27	84	1.09	.37	73	.98	.33	82

Av., average reaction time. m.v., mean variation. No., number.

TABLE XII

(Double Task Method)

RELATION BETWEEN THE OBSERVER'S REACTION-TIME IN SECONDS AND THE KIND AND DIRECTION OF THE OBJECTIVE CHANGE

O.	KIND						DIRECTION					
	INTENSITY			EXTENSITY			DOWN			UP		
	Av.	m.v.	No.	Av.	m.v.	No.	Av.	m.v.	No.	Av.	m.v.	No.
G	1.31	.78	34	1.05	.47	39	1.21	.68	40	1.14	.57	33
C	1.66	.78	24	1.66	.90	17	1.55	.89	20	1.77	.89	21
Gn	1.08	.29	30	.98	.19	27	1.07	.27	18	1.02	.22	39

Av., average reaction time. m.v., mean variation. No., number.

Tables II and IV show that there is a positive correlation between variation in attention and accuracy of work, regardless of the kind and direction of the objective change. We have seen, nevertheless, from Tables VII and VIII, that intensive changes are more compelling than extensive, and that changes to a greater are more compelling than changes to a smaller. This relation is only partially confirmed by the reaction times, as is shown by Tables XI and XII above. If we assume that the most compelling changes give the shortest reaction time, then we must say that the reaction times show that extensive changes and changes to the greater are the most compelling, since the average reaction times and mean variations are larger for intensity than for extensity and larger for changes to a smaller than to a greater. The relation is not absolute, for C in the Double Task Method has the same average reaction time for both the intensive and extensive changes, and a shorter reaction for the changes to a smaller than to a greater. The difference is, however, in no case great and shows once more that the work in these experiments was not influenced by anything else than fluctuation of attention.

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[32] P. 486. [33] P. 502.

(5) A comparison of Tables VII and XI with Tables VIII and XII shows that there is a close correlation between the *reaction time and the accuracy of report*. This relation is more clearly shown in Tables XIII and XIV (*cf.*, in the first paper, Tables VII[34] and XVI[35] respectively), in which the reaction times are compared directly with the accuracy of the reports. The results agree with those previously reported.

TABLE XIII

(Single Task Method)

THE REACTION-TIME IN SECONDS AND THE MEAN VARIATION OF THE RIGHT, HALF-RIGHT, AND WRONG REPORTS

Observer	RIGHT			HALF-RIGHT			WRONG		
	Av.	m.v.	No.	Av.	m.v.	No.	Av.	m.v.	No.
G	1.27	.33	127	1.33	.35	33	1.44	.15	9
C	1.38	.41	111	1.50	.45	26	1.84	.70	12
Gn	.97	.28	108	1.21	.49	36	1.26	.37	11

Av., average reaction-time. m.v., mean variation. No., number.

TABLE XIV

(Double Task Method)

THE REACTION-TIME IN SECONDS AND THE MEAN VARIATION OF THE RIGHT, HALF-RIGHT, AND WRONG REPORTS

Observer	RIGHT			HALF-RIGHT			WRONG		
	Av.	m.v.	No.	Av.	m.v.	No.	Av.	m.v.	No.
G	0.71	.19	15	1.18	.58	40	1.62	1.02	18
C	1.36	.58	10	1.48	.76	24	2.33	1.08	7
Gn	0.98	.30	20	1.05	.23	13	1.06	.19	24

Av., average reaction-time. m.v., mean variation. No., number.

(6) The *effect of the distractors* used in the Single Task Method is shown in Table XV (with which *cf.* Table VIII of the other study[36]).

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[34] P. 486. [35] P. 503.

[36] P. 487, with which *cf.* p. 472.

TABLE XV

(Single Task Method)

THE AVERAGE CLEARNESS OF THE CUTANEOUS SENSATIONS AS AFFECTED BY THE DISTRACTORS

O.		Distractor						
		0	1	2	3	4	5	6
G	Av. C.	85.5	81.6	84.3	83.8	82.9	76.3	73.5
	m.v.	4.1	9.2	7.4	7.2	5.8	12.2	13.0
	No.	30	22	31	33	31	18	34
C	Av. C.	87.2	75.0	84.0	70.2	85.0	77.1	65.0
	m.v.	14.3	22.3	16.4	22.0	13.2	20.4	27.1
	No.	36	22	31	36	31	18	34
Gn	Av. C.	94.7	70.0	78.8	71.1	85.3	77.6	82.5
	m.v.	1.7	14.8	7.2	13.3	7.0	6.8	8.7
	No.	36	22	31	36	31	18	34

Av. C., average clearness of cutaneous sensations. m.v., mean variation. No., number of cases. 0, normal conditions. 1, flicker. 2, buzzer. 3, bell. 4, metronome. 5, flicker and buzzer. 6, phonograph.

The table shows that the distractors were not especially effective, that there was a wide variation from day to day in their effectiveness, and that there was a great difference in their effect upon the different observers. We have already discussed the factors upon which these points depend in our previous paper.[37] The last point is exhibited in Table XVI (corresponding to Table IX of the first paper [38]).

TABLE XVI

(Single Task Method)

THE ORDER OF THE EFFECTIVENESS OF THE DISTRACTORS FROM LEAST TO GREATEST AS DETERMINED BY THE AVERAGE CLEARNESS OF THE CUTANEOUS SENSATIONS

Observer	Order						
	1	2	3	4	5	6	7
G.	0	2	3	4	1	5	6
C.	0	4	2	5	1	3	6
Gn.	0	4	6	2	5	3	1

0, normal. 1, flicker. 2, buzzer. 3, bell. 4, metronome. 5, flicker and buzzer. 6, phonograph.

(7) The *effect of the secondary tasks upon attention* is shown in Table XVII, which corresponds to Table XVII of our earlier work.[39]

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[37] Pp. 489f. [38] P. 488. [39] P. 504.

TABLE XVII

(Double Task Method)

THE AVERAGE CLEARNESS OF THE CUTANEOUS SENSATIONS AS AFFECTED BY THE DIRECTION OF ATTENTION ON THE PRIMARY OR SECONDARY TASK

Observer		ATTENTION DIRECTED TO		
		0	1	2
G	Av. C.	92.0	22.5	44.3
	m.v.	6.30	15.19	25.47
	No.	36	75	69
	Av. R.	1.02	1.32	1.11
C	Av. C.	95.0	32.8	23.9
	m.v.	4.30	15.57	19.57
	No.	36	75	69
	Av. R.	1.10	1.57	1.69
Gn	Av. C.	95.0	18.4	21.2
	m.v.	3.33	8.56	11.45
	No.	36	75	69
	Av. R.	.78	1.12	.90

Av. C., average clearness of cutaneous sensations. m.v., mean variation. No., number of cases. 0, attention directed to cutaneous sensations. 1, attention directed to adding processes. 2, attention directed to subtracting processes. Av. R., average reaction time for experiments under each rubic.

The results are like those of the previous paper. They show the effect of the division of attention. In the normal or control experiments, the average clearness of the cutaneous sensations is uniformly higher, the mean variation is uniformly smaller, and the reaction time is uniformly shorter than is the case in the Double Task experiments.

(8) Table XVIII shows the *relation between the character and quality of the work performed, and attention* as introspectively estimated in terms of attributive clearness (Table XVIII of the first article [40]).

TABLE XVIII

(Double Task Method)

COMPARISON BETWEEN THE AVERAGE CLEARNESS OF THE MENTAL PROCESSES INVOLVED IN CONTINUOUS ADDING; IN CONTINUOUS SUBTRACTING; AND THE ACCURACY OF THE WORK PERFORMED

O.	Report	MENTAL PROCESS											
		ADDING—CLEARNESS						SUBTRACTING—CLEARNESS					
		10-9	9-8	8-7	7-6	6-5	5-4	10-9	9-8	8-7	7-6	6-5	5-4
G	Correct	2						4	1				
	Nearly correct	4	2	2				6	1				
	Failure	3	3	4	3	2		1	3		2	5	
	No. cases	9	5	6	3	2		11	5		2	5	
C	Correct	2											
	Nearly correct	4	3	1	1	1		4	2				
	Failure	4	1	5		3		4	2	3	2	6	
	No. cases	10	4	6	1	4		8	4	3	2	6	
Gn	Correct	1	1										
	Nearly correct	2	3	1				1	1				
	Failure	3	10	1	2	1		4	7	7	2	1	
	No. cases	6	14	2	2	1		5	8	7	2	1	

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[40] P. 504.

The results are those that we have learned to expect; more correct answers and fewer failures were returned during the higher degrees of clearness than in the lower. We have evidence once more, therefore, that introspectively distinguished variations of clearness are closely paralleled by corresponding differences in the accuracy of the work performed.

(9) In working over the data for information concerning the *number of levels* of attentive consciousness, we have included the introspections of L. B. Hoisington, given during the preliminary training and practice periods. H's work was cut short by mid-year graduation, and his results were for that reason not reported in the other connections. He gave in the preliminary training and practice periods 189 reports; which, with those of the other observers, make up a total of 1175.

The observers reported without exception the dual division of consciousness; that is to say, a clear focus and a vague background, which varied reciprocally.[41] The introspective values for the upper and lower levels of clearness did not in every case total 100, but the discrepancy was never very large. It was never more than five degrees the one way or the other, and more often smaller than larger.

SUMMARY

Our conclusions, based upon the present work with extent and intensity of cutaneous sensation, substantiate the conclusions which we have printed for the work with pitch and intensity of auditory sensation.[42] The reader is referred to the previous Summary.

We find again that the Double Task or division method gives a greater variation in the range of attention than does the Single Task or distraction method.[43] Thus we may conclude that the Double Task method is the more effective.

Concerning the relation of intensive to extensive changes, we may now conclude that intensive changes and changes to a greater intensity or extent are slightly more compelling than are extensive changes and changes to a smaller intensity or extent.

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[41] *Op. cit.*, p. 506. [42] *Op. cit.*, 507. [43] *Op. cit.*, 490.

VISUAL IMAGERY AND ATTENTION: AN ANALYTICAL STUDY[1]

By Helen Clark

I. Introduction

Ever since the appearance of Galton's "Inquiries into Human Faculty,"[2] the subject of imagery has received almost constant attention from psychologists, and, moreover, its cultivation has been strongly marked by an increasing diversity of new problems. It is not necessary to pass the general literature of the subject in review; but we shall briefly mention those phases of it which are most closely related to the present study. First of all, we must note that the current tendency to view mind in its historical aspects has prompted the psychologist to seek for the *antecedents* of the image; to discover, that is to say, its dependence upon perception. Experimentalists now generally "incline to the view that there is no intrinsic difference between sensation and image."[3] Investigations in the field of suggestion reveal the fact that, under certain circumstances, simple or total images may be mistaken for sensations or perceptions.[4] On the other hand, "a visual perception of distinctly supraliminal value may . . . pass—even with specially trained observers—for an image."[5] Related studies have dealt with the *development* and the *decay* of imaginal complexes in the course of time. The associationists' conception of stable psychical entities has been discarded, and the view commonly held at present is that "the memory is not to be regarded as a storehouse of perfectly conserved images, but that the most simple memories are continually exposed to change, and that it is, at times, only by the combination of various memorial resources that retention is made definite and exact."[6] Experimental and theoretical inquiries have also been directed toward the *general functions* of the image in relation to thought and meaning,[7] discrimination and judgment,[8] and reproduction and recognition.[9] Moreover, there have been several attempts made at *classification* of total images on a basis of divers characteristics, usually functional. Visual images have here received most consideration. Four important investigations which have recently appeared are those of Perky,[10] L. J. Martin,[11] K. Koffka,[12] and R. M. Ogden.[13] And finally, images have been studied in their relations to motor phenomena, especially to *ocular movement.*[14]

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[1] From the Psychological Laboratory of the University of Illinois.

[2] Galton, F., Inquiries into Human Faculty and its Development, 1883.

[3] Perky, C. W., An experimental study of imagination, *Amer. J. of Psychol.*, 1910, xxi, 435. Cf. Külpe, O., *Philos. Stud.*, xix, 508ff., and Washburn, M. F., *Mind*, n. s., viii, 1899, 32.

[4] Cf. Seashore, C. E., Measurements of illusions and hallucinations in normal life, *Studies from the Yale Psychological Laboratory*, 1893-97, i-v, 32.

[5] Perky, *op. cit.*, p. 435.

The problems and methods in Ogden's and in Perky's investigations closely resemble those of our own experiments. There are, however, three important differences. (1) Both Ogden and Perky discovered a number of intermediate or " equivocal " images[15] which did not seem to belong either among their " images of imagination " or with their " images of memory." We decided to include in our classification all non-verbal (or primary) images which were reported by our observers. (2) As regards ocular movement, Ogden obtained negative results (p. 381); while Perky, by means of another procedure, discovered a striking correspondence between the kind of image and the occurrence of ocular movements (pp.

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[6] Bentley, M., The memory image and its qualitative fidelity, *Amer. J. of Psychol.*, 1899, xi, 47f. Cf. Philippe, J., L'image mentale; evolution et dissolution. Paris, 1903, 76ff, 116f.

[7] Cf. Betts, G. H., *The distribution and functions of mental imagery.* Teachers College, Columbia University, 1909, p. 41.

[8] Cf. Whipple, G. M., An analytical study of the memory image and the process of judgment in the discrimination of clangs and tones, *Amer. J. of Psychol.*, 1901, xii, 409ff.

[9] Bentley, M., *op. cit.*, 46.

[10] *Op. cit.*

[11] Die Projecktionsmethode und die Lokalisation visueller und anderer Vorstellungsbilder, *Zsch. f. Psychol.*, 1913, lxi, 321.

[12] *Zur Analyse der Vorstellungen und ihrer Gesetze*, Leipzig, 1912.

[13] Experimental criteria for differentiating memory and imagination in projected visual images, *Psychol. Rev.*, 1913, xx, 378.

[14] Perky, *op. cit.*, and Ogden, *op. cit.* Cf. Meakin, F., Mutual inhibition of memory images, *Harvard Psychological Studies*, i, 1903, 244; Moore, C. S., Control of the memory image, *ibid.*, 296; Slaughter, J. W., Behavior of mental images, *Amer. J. of Psychol.*, xiii, 1902, 548; Kuhlmann, F., Analysis of the memory consciousness, *Psychol. Rev.*, xiii, 1906, 338f; Murray, E., Peripheral and central factors in memory images, *Amer. J. of Psychol.*, xvii, 1906, 241; Külpe, *Outlines*, 1909, 187; Burtt, H. E., Factors which influence the arousal of the primary visual memory image, *Amer. J. of Psychol.*, xxvii, 1916, 87.

[15] Ogden, p. 406; Perky, 436.

442, 451). We decided to observe movements of the eyes by three different methods, one of them being somewhat similar to Perky's. (3) We proposed to discover, if possible, the *factor or factors most closely connected with ocular movements,* not simply the sort of image which was usually involved.[16]

Before proceeding to the account of our experiments, we must observe that the term "image" is ambiguous. As it is used by psychologists to-day, it stands for (1) a mental process, (2) a group or constellation of mental processes, (3) a kind of reference or meaning, and (4) a particular sort or class of objects. As regards the first two meanings, we shall distinguish the "simple image" (a single imaginal process taken at the level of sensation and affection) from the "total image" (the single imaginal constellation or complex); and so far as the functional treatment of imagery is concerned,—the treatment which discovers such marks as "particularity," "locality," and "personal reference,"—we shall try to make clear in the context when we are dealing with meanings and objects and when we intend to indicate processes. The attempt is worth while because it is still frequently assumed, even by serious writers, that the imaged object is a "mental" object and so ultimately different for the psychologist from the physical objects of perception.

II. EXPERIMENTAL: AN ANALYSIS OF VISUAL IMAGERY

Problem and method. Our first investigation was an analytical study of the visual images aroused by a series of words. The purpose of the experiment was to obtain a descriptive account of the images evoked with respect to twelve different characteristics, to classify the images with reference to two of these peculiarities, and to discover whether there was a resemblance in other details among the images of each group.

There were two observers, A and B. O[17] was seated in a dark room facing a black wall. The experimenter took her place behind the observer at a table provided with a shaded light for the recording of O's descriptions. The experimenter transcribed O's account as O dictated. For evoking the images, 114 substantives were used with A; with B, 75 words taken from the same list.[18] The instructions were as follows: "After a signal 'ready,' I shall pronounce a word. You are to note any visual image that may occur. As soon as it has disappeared, give a full description of it."

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[16] Our observers, named in the order in which they served in the experiments, were: an advanced undergraduate—Miss B. V. Copley (A); two graduate students—Mr. C. E. Holley (B), and Miss A. S. Rogers (C); three instructors in psychology—Dr. C. A. Ruckmich (D), Dr. J. E. DeCamp (E), and Dr. C. Rahn (F). The author wishes to express her grateful appreciation of their services and to acknowledge her especial indebtedness to Professor Bentley, under whose supervision the investigation was pursued.

[17] "O" has been used in the following pages as an abbreviation for "observer."

It is to be observed that the instructions were quite general. The subject was not *required* to call up an image, but merely to note any that should occur. Furthermore, the image was not supposed to be that of a particular object, but simply any visual image. The subject was not asked voluntarily to hold the image, which was to take its natural course.

The description was not interrupted; but, when O had finished his account, he was questioned as to any points which he had overlooked. The experimenter sought complete information concerning (1) specificity of reference ("particularity"), (2) familiarity, (3) the position of the imaged object in relation to O, (4) the visual background, (5) the size of the imaged object, (6) its stability, (7) the number and the degree of clearness of its details, (8) color, (9) accompanying affective tone, (10) somatic reference, (11) associative processes, and (12) temporal reference.

Results. The images reported were first divided into three main groups. Those which referred to *specified objects* and which were apprehended as *familiar* were called *F-images.* Memory images (those which not only referred to specified objects and were apprehended as familiar, but which also involved a reference to a particular place and to O's past experience), were considered as a sub-class of the F-images. Images which were *specific* and *unfamiliar* were called *U-images.* These were not necessarily accompanied by a feeling of strangeness; there was, in most cases, merely absence of familiarity. Finally, images of *general reference* (signifying a class or a member of a class of objects), which also involved no familiarity, were designated as *G-images.*[19] With regard to position, the imaged objects were classified as *usual* when they had the same position in relation to the observer as the perceptual objects commonly occupied; they were called *unusual* if the position was not characteristic of the perceptual object, or if the imagined object seemed detached from any setting, or if the localization was extremely indefinite. Images were classified as *unstable* if they fluctuated, faded quickly, or were soon replaced by other images. When describing the

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[18] Some of the words were: knife, river, dog, church, soap, cloud, grass, king, lyre, Cerberus, druid, tournament, King Arthur, wand, nightingale, baron, Marmion.

[19] Both the generality of reference and the definiteness of spatial orientation we found, as did Koffka, to present a large number of degrees and gradations. These differences seem, however, to be unimportant for our main purpose.

details of images as *many* or *few,* the experimenter took into consideration the amount of detail which corresponding objects revealed in perception. *Color* was described as *absent* if color was normally present in the perceptual mode of the object. We took *associative processes* to mean relevant processes not completely assimilated to the total image. Such associative processes might occur at the same time as the principal image, although they were more apt to appear subsequently. Where the corresponding perception had occurred at some time within a year, the image was classed as *recent*. *Somatic reference*[20] was present in several forms. In some cases, the observer simply was at the place presented in imaginal form.

"The church was in the position in which it would be if I were walking up to it." "Then the image changed as I went inside." "I was there. I seemed to be younger than I am now." "I was lying on my back and looking at the clouds."

Some images stood in definite relation to certain parts of the body.

"The gun was parallel to my arm as though my arm were extended." "The bird was back of my arm at the left. I thought of turning my head and looking at it." "The shoes were on my own feet."

In some cases, the somatic reference was more explicit.

"I imagined myself as there." "I seemed to be coming along the walk." "I knew that I was there."

In one case, the subject had a definite visual image of herself.

"I saw myself take the key out of the rubber where it is kept."

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[20] The word "somatic" is used to avoid the ambiguous term "personal." Perky uses, without definition (p. 436), "personal reference" as one of her two cardinal distinctions between "images of memory" and "images of imagination." We mean by "somatic reference" that the observer apprehended the imaged object as related to his own perceived or imaged body. The words "personal reference" might have a similar significance; but they might also imply a reference to the observer's past experience. Our own analyses seem to indicate that "particularity," Perky's second cardinal distinction, is—even when taken with the first—an unsatisfactory mark of memory. We found many particular and specified objects which could not, save by the most arbitrary designation, be called "memories." Similar criticism upon these terms has been passed by Koffka (p. 226) and Ogden (p. 384ff). Perky's determination to study "imagination" appears to have induced her to accept, without refined analysis, a "rough and ready criterion" of the simpler imaginal complexes. Although she uses this criterion "for preliminary purposes" (p. 451), it nevertheless determines her whole conception of the simpler processes and modes of integration by which, as she assumes, "imaginations" are built up.

All of these functional distinctions are, of course, more or less crude. They should, however, suffice to reveal any striking dissimilarities existing among our classes.

Table I shows the distribution of the different kinds of images for each observer.

TABLE I

Observers	F-images	G-images		U-images	Totals
	Primary	Primary	Secondary	Primary	
A	36	61	..	43	140
B	36	12	7	4	59
Totals	72	73	7	47	199

Most of the visual images were primary (non-verbal), but some of B's images were secondary or symbolic (verbal). The 7 secondary visual images mentioned in the table appeared as independent total images. They are not included in the following discussion of results unless specifically mentioned.

Table II shows a more detailed analysis of the images. As regards the temporal relations of the F-images, over half (53% and 61%) were referred to objects seen habitually and recently (within a year), while more than a third (36% and 39%) referred to objects perceived on some recent and specific occasion. In 11% of A's F-images there was recognition without definite temporal relations. Under three of the principal headings of classification there seemed to be little or no correspondence between the kind of imagery and a particular characteristic. These headings were color, stability, and size.

1. *Color* was present in all but six images. Three of these were F-images, one was a G-image, and one was a U-image.

2. *Stability* was characteristic of 61% of A's F-images, but only 39% of B's F-images were stable. Of B's G-images 83% were unstable. In the other cases, the percentages are not significant.

3. *Normal size* was reported in from 70% to 100% of the cases in each group. In the case of A, the G-images characterized by abnormal size were three times as numerous as the corresponding F-images, while the abnormal U-images were four times as numerous as the F-images. Of B's images, on the other hand, the F's were the only ones that were ever characterized by abnormal size. These reports, however, may

TABLE II

			Position		Background				Size			Stability		Details and Clearness				Color		Affective Tone			Somatic Ref.		Associative Processes			Temporal Relations		
			Usual	Unusual	Lacking	Color—Shade—Haze	Vague Scene	Clear Scene	Exaggerated	Normal	Dwarfed	Stable	Unstable	Many—Clear	Many—Vague	Few—Clear	Few—Vague	Present	Lacking	Pleasant	Neutral	Unpleasant	Present	Lacking	Present	Lacking	Only Image of Stimulus Word	Recent—Habitual	Recent—Specific	Uncertain
-IMAGES	A	No.	32	4	4	2	8	22	3	33	....	22	14	26	3	4	3	36	....	26	4	6	23	13	13	23	....	19	13	4
		%	89	11	11	6	22	61	8	92	....	61	39	72	8	11	8	100	....	72	11	17	64	36	36	64	....	53	36	11
	B	No.	35	1	3	2	20	11	....	35	1	14	22	11	5	7	13	33	3	17	19	....	22	14	13	13	10	22	14	....
		%	97	3	8	6	56	31	....	97	3	39	61	31	14	19	36	92	8	47	53	....	61	39	36	36	28	61	39	....
[illegible]	[illegible]	No.	37	24	12	28	17	4	10	46	5	29	32	19	1	23	18	60	1	27	20	14	7	54	9	52	....	....	....	....
		%	61	39	20	46	28	7	16	75	8	48	52	31	2	38	29	98	2	44	33	23	11	89	15	85	....	....	....	....
	[illegible]	No.	4	8	11	....	1	....	....	12	....	2	10	....	1	....	11	12	....	....	12	....	1	11	3	7	2	....	....	....
		[illegible]	33	67	92	....	8	....	....	100	....	17	83	....	8	....	92	100	....	....	100	....	8	92	25	58	17	....	....	....
	[illegible]	[illegible]	36	7	12	2	19	10	2	30	11	22	21	26	3	6	8	42	1	27	4	12	6	37	29	14	....	....	....	....
		[illegible]	[illegible]	16	28	5	44	23	5	70	26	51	49	60	7	14	19	98	2	63	9	28	14	86	67	33	....	....	....	....
	[illegible]	[illegible]	[illegible]	1	2	....	1	1	....	4	....	2	2	1	2	....	1	3	1	1	3	....	2	2	2	2	....	....	....	....
		[illegible]	[illegible]	25	50	....	25	25	....	100	....	50	50	25	50	....	25	75	25	25	75	....	50	50	50	50	....	....	....	....

be significant when considered in another connection.[21] Table III shows that it was usually the images of large objects that were under-sized and also those of small objects that were over-sized.

TABLE III

Images of	Exaggerated	Dwarfed
Small objects	10	1
Medium objects	2	..
Large objects	3	16

The small objects which were exaggerated when imaged were a picture of Queen Elizabeth, a pin, a lemon, a key, a cake of soap, a dinner favor, a tomato, a maple leaf, and two flies. The image of a snake was under-sized. The two medium-sized objects which were exaggerated in imagery were a coat and a bag. Of the large objects, a king, a giraffe, and Ceres were exaggerated, while a tower, a valley, a castle, a horse, a temple, a tabernacle, King Arthur, a crusader, a baron, a serf, Marmion, the Delphic oracle, the Nile, and three forest scenes were under-sized when imaged. With 15 of the 17 images that were dwarfed there was a visual background. With 11 of the 15 that were exaggerated there was no visual background or only a shade or haze, while in 2 more cases the scene was vague.

Objects of a certain size are probably perceived with less effort than objects or scenes considerably larger or smaller. Perception of the small objects may involve unpleasant muscular strain resulting from the convergence of the eyes. This is especially true when the object is close to the observer. Large objects or scenes necessitate considerable ocular movement if all the different details are to be clearly perceived. In some cases, images seem to show a tendency to take that size which, in actual perception, would involve the least ocular strain.

Although the three kinds of images did not present marked differences as regards color, stability, and size, certain other peculiarities were more characteristic of one sort of imaged object than of another. We distinguish six cases.

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[21] J. M. Baldwin and W. J. Shaw (Memory for square size, *Psychol. Rev.*, ii, 1895, 236) found that the size of comparatively small objects was over-estimated. H. K. Wolfe (Some judgments on the size of familiar objects, *Amer. J. of Psychol.*, 1898, ix, 137) discovered that familiar objects (*e. g.*, bank-notes) were under-estimated when drawn on paper. In criticism of this experiment it has been said that "this fact does not necessarily report the visual image of the note, since reproduction might easily be biased by the introduction of alien senses, *i. e.*, pressure, muscular exertion, etc." (M. Bentley, The memory image and its qualitative fidelity, *Amer. J. of Psychol.*, 1899, xi, 1.)

1. The F-images occupied a "usual" *position* (89% and 97%); the U-images not quite so frequently (84% and 75%). A's G-images were less apt to occupy a usual position (61%), and, in the case of B, the position was even less likely to be usual (33%).

2. The *visual background* in 83% and 87% of the F-images and in 67% and 50% of the U-images was a scene. On the other hand, there were scenic backgrounds in only 35% and 8% of the G-images.

3. The *details* of the images were both many and clear in 72% (A) and 31% (B) of the F's and in 60% (A) and 25% (B) of the U's. The details were few and vague in 29% (A) and 92% (B) of the G-images.

4. *Somatic reference* occurred with 64% and 61% of the F-images, but with only 11% and 8% of the G-images and with 14% and 50% of the U-images.

5. *Associative processes* were present with a considerable number of U-images (67% and 50%), with a smaller number of F-images (36% and 36%), and with still fewer G-images (15% and 25%).

6. As regards *affective tone*, F-images were often accompanied by pleasantness (72% and 47%), U-images were less frequently associated with pleasantness (63% and 25%), while with G-images consciousness was even less often pleasant (44% of A's images) and might be predominantly neutral in tone (100% of B's images). B reported no cases of unpleasant affective tone. Of A's 140 images, 32 were connected with unpleasantness. In 17 of these cases, however, the unpleasantness might be explained by the nature of the object imaged, while in 6 other cases the unpleasant tone is to be referred to associative connections. *Only two images involved a definite feeling of strangeness or unfamiliarity.* One of these was accompanied by pleasantness, the other by unpleasantness. These results would seem to show that images referring to agreeable experiences are more apt to be recalled than are those referring to the disagreeable.[22]

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[22] The belief that there is a tendency to forget the disagreeable is held by Colegrove (*Memory*, 1900, 11), Freud (*Zur Psychopathologie des Alltagslebens*, 1907), and Hollingworth (The oblivescence of the disagreeable, *J. of Phil., Psychol.*, etc., 1910, vii, 709). This view is questioned by E. N. Henderson (Do we forget the disagreeable? *ibid.*, 1911, viii, 432), who found that one-third of the memories of ten observers were disagreeable. He considers it possible that the average person actually has more pleasant than disagreeable experiences, and that an incident which is remembered with pleasure might really have involved unpleasantness when it occurred. "We forget not so much dis[illegible] s."

A certain *combination of characteristics* might be regarded as peculiar to the *pure type* of each sort of image. The *pure F-image* may be distinguished by its usual position, its scenic background, many clear details, a pleasant affective tone, somatic reference, and associative processes. The *pure G-image* is in an unusual position and has no background, or merely a shade or haze as background, few and vague details, a neutral affective tone, no somatic reference, and no associative processes. Like the F-image, the *pure U-image* has a usual position, a scenic background, many clear details, a pleasant affective tone, and associative processes; but it has no somatic reference. If, however, one attempts to classify the individual images under these detailed headings, it is evident that there are very few examples of the pure types and many deviations from them.

Table IV summarizes the variations of the images from the pure types. Only 17 of the whole number are "pure" under our definitions, while 48 vary from the "type" in one, and 68 in two particulars. Transition from one class to another was shown in various ways. Let us illustrate. In connection with one image there was a feeling of familiarity, but the observer was unable to place it in any spatial or temporal relations. In other cases there was no recognition, though scenes associatively appeared which the observer thought might possibly have been the original settings of the object. In four cases recognition occurred after the image had begun to fade. Several G-images and one U-image had as backgrounds scenes that were recognized. Once the image remained the same while one scenic background was replaced by another. In 44 cases the pronunciation of the word was

TABLE IV

	A's Images			B's Images		
	F-Im.	G-Im.	U-Im.	F-Im.	G-Im.	U-Im.
Pure type................	4	1	7	1	4	0
Variant in 1 characteristic	11	13	10	9	5	..
2 characteristics.......	14	16	15	19	3	1
3 characteristics.......	4	15	10	4	..	1
4 characteristics.......	1	13	1	23	..	2
5 characteristics.......	1	3	..	..	..	..
6 characteristics.......	1	..	..	1	..	..

followed by two successive total-images. Of these successions nine were from one G-image to another of the same sort, nine were from an F-image to another F-image, and nine were from a G-image to a representative of the F class. In 13 cases, images from other sense departments were assimilated to the total image. Five of these were temperature-tactual images, 5 were kinaesthetic-tactual, 2 were olfactory and one was auditory.

So far as it is permissible to base general conclusions upon our material, these results seem to indicate that, although visual images may be classified according to their genesis or function, there are no definite groups which show peculiar patternings of the ten characteristics, intrinsic and functional, which we have mentioned. Individual differences are considerable, and intermediate images are very numerous. F-images, G-images, or U-images may, perhaps, best be considered as designating *various points in a developmental or an elaborative series, rather than sharply differentiated classes.*

III. EXPERIMENTAL: IMAGERY, OCULAR MOVEMENT AND ATTENTION

Experiment 1

Problem and method. The purpose of this experiment was to discover whether there exists any connection between the general character and functions of the visual image and the occurrence and extent of ocular movements. A and B again served as observers. The observer was seated in the dark room with his head fixed in a head-rest. A sheet of blank cardboard was placed at the distance of a meter from his eyes so that a white fixation-point appeared opposite the fovea of the right eye. The experimenter, who was seated at the observer's right, observed the latter's eye through a hand lens. A black box with a very narrow slit in one side was so arranged that the light from an electric bulb inside produced a bright point of light upon the cornea. The position of the box was shifted until the point of light fell upon the iris of O's eye near the edge of the pupil. When the eye shifted, the margin of the pupil moved noticeably nearer to or farther away from the dot of light, which remained approximately stationary. For the practice series, a sheet of black cardboard was used with a horizontal and a vertical line crossing at right angles. Distances of 5 cm were laid off as units along each line. O looked at the fixation-point, then at some point along one of the lines, then again at the fixation-point. After practice, the experimenter was able to tell, with approximate accuracy, from the appearance of the eye, over how great a distance upon the cardboard the observer's glance had passed in a vertical or a horizontal direction. From the list used in the experiments of Chapter II, we selected the words which had aroused images differing but slightly from the pure types. The selection was made so that the proportion of images in each of the three classes should be approximately the same. Sixty-nine words[23] were pronounced for A, while only the first 35 were used for B. The instructions were those of the preceding experiments with the following additions: "You are to fixate the white dot upon the black screen. When a visual image appears, try to keep it constant, but, if it disappears, do not make an effort to recall it. You are to start the stop-watch when the image first appears, and to stop it when the image finally disappears." After the observer had given his account of the image, he was questioned as to any points which he had overlooked. The experimenter sought complete information concerning (1) the visual background of the imaged object, (2) its size and (3) details, (4) somatic reference, and (5) accompanying kinaesthetic processes.

Results. Table V gives a general tabulation of the images of observers A and B.

Some visual-verbal images occurred, but they are not included in these results. Reports are not included under every heading for every image, since in some cases the image would shift, or O would be uncertain as to an introspective item. The percentages are based upon the numbers tabulated.

As regards the nature of the different sorts of image, these results are, on the whole, in accord with those of the experiments already recorded. F-images and U-images generally had a scenic background, while, with G-images, an imaged visual background was either only a haze or was altogether lacking. In F-images the details were usually numerous and clear. This was less often the case with U-images. G-images were more apt to have few and vague details than were those of either of the other groups. Somatic reference was characteristic of F-images, while it did not seem to be present with the other kinds. In this experiment, U-images seemed

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[23] The list of words was as follows: sword, soap, ogre, leaf, ghost, orange, nymph, broom, pin, oracle, cat, tomato, druid, pipe, catacombs, horse, key, hamlet, convent, church, Marmion, lyre, snake, banana, ice, centaur, aeroplane, mirror, child, knife, serf, dungeon, stone, gun, tournament, boat, cloak, river, wagon, nail, tiger, helmet, coin, witch, moat, grave, wand, rose, dog, coat, cloud, ring, lion, grass, lemon, sled, hand, bag, chimney, purse, griffon, temple, shoe, Holy Grail, vase, Jupiter, sultan, shrine, Nile.

TABLE V

			Visual Background				Size			Details				Somatic Ref.		Kinaesthetic Sensations or Images				Ocular Movements	
		Number of Images	Black Screen Only	Lacking	Color—Shade—Haze	Scene	Exaggerated	Normal	Under-Sized	Many—Clear	Many—Vague	Few—Clear	Few—Vague	Present	Lacking	Lacking	Ocular Strain	Other Sensations	Images (Kin.)	Lacking	Present
F-Images........	No.	45	0	1	0	44	0	44	1	34	0	4	2	36	7	32	7	1	4	25	20
	%	...	...	2	...	98	...	98	2	85	...	10	5	84	16	73	16	2	9	56	44
G-Images........	No.	33	14	10	8	1	0	30	3	13	2	4	12	0	31	25	3	0	0	32	1
	%	...	42	30	24	3	...	91	9	42	6	13	39	...	100	89	11	...	...	97	3
U-Images........	No.	18	5	0	0	13	1	11	6	8	4	1	5	0	15	11	4	0	0	6	12
	%	...	28	...	...	72	6	61	33	44	22	5	28	...	100	73	27	...	...	33	66
Total........	...	96	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...

the most likely to be exaggerated or dwarfed. This was less often the case with G-images and it seldom occurred with F-images. Ocular movements and sensations of ocular strain seldom occurred in connection with G-images, but were frequently noticed with the other varieties. Table VI shows the number and the extent of ocular movements. Although these

TABLE VI

	Average number of movements per image	Average extent of movement (in cm.)	Right to Left		Left to Right		Up or Down	
			Total No.	Extent	Total No.	Extent	Total No.	Extent
F-images......	1.84	5.64±2.9	13	4.66	13	7.11	8	3.75
U-images......	1.33	5.15±2.4	6	3.33	9	6.44	1	2.5

movements were more frequently present with U-images than with F-images, the average number of movements per image was less and the extent was less in the former case than in the latter. "Extent" refers to the distance, as on the cardboard screen, over which the glance of the observer was estimated to have passed, not to the amount of movement of the eye-ball. Complete data concerning the duration of the images cannot be given, since 18 of the 33 G-images were immediately followed by F-images. In such cases, the duration of each could not be accurately determined. In 52 instances, however, the time-interval could be ascertained. These results are shown in Table VII. F-images and U-images were, in general, longer than G-images. The significance of the figures is dubious, however, because of the wide variations in time.

TABLE VII

	Number	Average duration	M. V.
F-images....................................	23	24.0 sec.	13.6
G-images....................................	35	10.8 sec.	8.1
U-images....................................	14	26.8 sec.	12.0

The fact that F-images and U-images are longer than G-images might account in some measure for the occurrence of ocular movements with the two former classes and their absence in other cases. The eyes might tend to move because there was long fixation, regardless of the nature of the accompanying image. The time-interval could not, however, be the sole factor involved, for ocular movements occurred with some very short F- and U-images, and did not occur with G-images which were considerably longer. Ocular movements occurred toward the end of 6 images which lasted longer than 15 sec., but in 3 cases other movements had also occurred toward the beginning of the period.

The results of this experiment seem to indicate that some relation obtains between ocular movement and different sorts of image. The existence of such a relation may seem strange in view of the results of our earlier experiment, which showed that, when grouped according to characteristics, there are few images of any pure type, each class tending to merge into the others. But the correspondence may be due to the fact that the amount of ocular movement accompanying any visual image is related to its general setting, rather than to the definite patterning of characteristics, which, as we have seen, varies considerably within the groups. It is possible that different characteristic motor attitudes or general conditions of the organism occur with F-images, G-images, and U-images, and that these attitudes involve certain reactions upon the ocular muscles. The fact that kinaesthetic sensations or images were reported in a comparatively small number of cases would not necessarily invalidate such a conclusion. The attention of the observer was directed toward the visual image, and the effects of muscular strain or movement might easily have been overlooked. The experimenter purposely refrained from all unnecessary emphasis upon this aspect of the problem in order that conditions might be as natural as possible.

Experiment 2

Problem and apparatus. It occurred to the writer that the results of Experiment 1 would be more significant if confirmed by investigations involving an objective record of ocular movements. It also seemed advisable to try to find the particular factor or factors which were most closely connected with ocular movement, instead of resting content with the discovery of the association of such movement with certain kinds of imagery. These were our chief purposes in the present experiment. For recording ocular movement, we employed the writing tambour of the laryngeal recorder described by Krueger and Wirth[24] which was connected by rubber tubing to the nystagmograph of A. Schackwitz.[25] A record of the time in seconds was kept by means of an electric marker actuated by a metronome, or, in the later experiments, by a time-clock. A second pneumatic system was used to record the course of the image. For this purpose a rubber bulb was so fitted into a cardboard frame that O could produce variations in the height of the recording line by changing the degree of pressure upon the bulb. Strong pressure meant a high degree of clearness in the imaginal processes.

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[24] *Psychol. Stud.*, 1905-06, i, 103.

[25] *Zsch. f. Psychol.*, 1913, lxiii, 442. When the spectacle-frame to which the tambour was attached proved to be unsatisfactory, we removed the tambour and fastened it to a support made of a system of adjustable rods and attached to the head-rest.

Method. C, D, and E served as observers. The observer was seated with his eyes closed and his head fixed in a head-rest. The first pneumatic system was inflated, and the small tambour of the second system, adjusted to the head-rest, was placed against the right eye-lid. Movements of the eyes were thus automatically recorded beside the clearness-line upon the kymographic paper. The instructions were as follows: "After a signal 'ready' a word will be pronounced. If a visual image appears, try to hold it constant, but do not recall it when it disappears. With the right hand, compress the bulb when the image appears. Increase the pressure whenever the image grows clearer and diminish the pressure whenever the image becomes less clear. After the image has disappeared, you will be interrogated as to (1) shifts in attention from object to object, (2) changes in the clearness, the intensity, and the content of the imaginal object, (3) associative processes, (4) somatic reference, (5) the visual background of the image, (6) specificity of the imaged object and its temporal and spatial relations, and (7) familiarity." Sixty-three words were used with each of the three observers.[26]

Results. Nothing of importance resulted from this experiment in respect to the characteristics of the different sorts of images. The descriptions are in general accord with the more detailed accounts discussed above. The results are significant, however, in that they show an appreciable correspondence between fluctuations in the clearness of the imagery and ocular movements. The records were divided into 5 classes according to the degree of correspondence. We described a record as showing no correspondence when one line fluctuated and the other did not, or when there were striking differences in the number and the position of fluctuations. Figure 1 shows typical examples of the records in which there was correspondence. Records 1, 2, and 3 are samples of high correspondence, record 4 of close correspondence, while in 5

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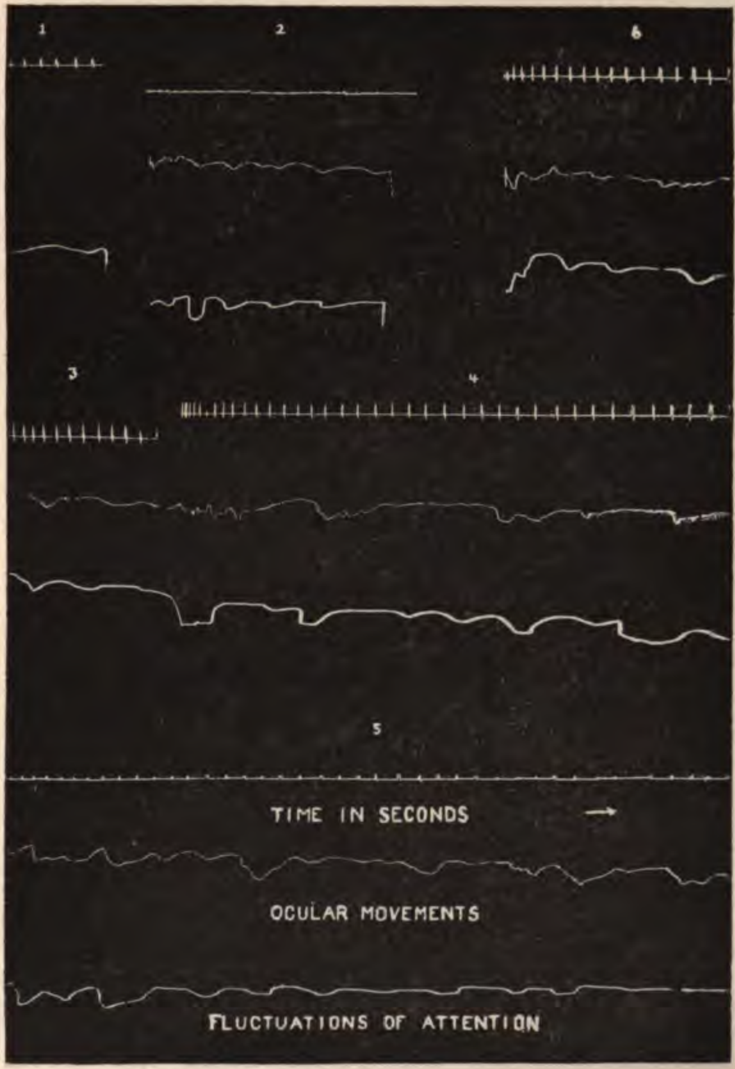
[26] Some of the words were as follows: pickle, wizard, maypole, shell, moat, Hercules, shepherd, thimble, rainbow, scythe, commencement, dell, note-book, Scotchman, Cinderella, mouse.

Fig. 1


1
2
6
3
4
5
TIME IN SECONDS →
OCULAR MOVEMENTS
FLUCTUATIONS OF ATTENTION

the resemblance of the lines is general, and in record 6 the correspondence is limited. Table VIII shows the classification of the results for each observer.

TABLE VIII

	Amount of Correspondence					
	None	Some	General	Close	High	Totals
C.	17	8	24	6	10	65
D.	17	21	9	5	8	60
E.	10	19	7	7	4	47
Totals	44	48	40	18	22	172
		128				

In 128 of the 172 cases, there appeared, then, a correspondence between the records for fluctuations of the clearness of the image and the records for ocular movements. This agreement is the more striking in view of the fact that very slight variations in the adjustment of the apparatus or in the time of O's response in varying the pressure upon the recording bulb might have changed the records in such a way that a correspondence which really existed would have been obscured.

Experiment 3

Problem and apparatus. In all of the preceding investigations, the images evoked differed widely in their nature and in their origin. Some were very schematic, while others presented many specifying details. A considerable number referred to recent experiences, but some were related to habitual perceptions, and still others symbolized classes of objects. In view of these facts, it seemed advisable to investigate the occurrence of ocular movement in connection with images resulting from similar perceptions. We decided to employ a series of objects which were homogeneous except in respect to meaning, and to note whatever differences in ocular movement might occur when the objects were imaged. For this purpose, we devised a series of thirty sense and nonsense figures. Each figure or diagram was cut from white paper and pasted on a black card 5 inches square. Half of the pictures represented meaningful objects, while the others were intended to be meaningless.

Ocular movements were detected by a method different from the methods used in our previous investigations. In Experiment 1, changes in the position of the eye had been observed by the experimenter; in Experiment 2, a mechanical device had been employed. In the following series, the observer himself reported the occurrence of ocular movement. Our plan was to place before the observer a set of points of light arranged in such a way that, when fixation was steady, all of the lights would fall upon the blind-spot. A slight movement of the eye, however, would bring some of the bright points within the field of vision.[27] The apparatus (represented in Figure 2) consisted of two black metal light-boxes designed for the investigation of indirect vision. The smaller light-box supplied the fixation-light and the larger box the lights for determining the limits of the blind-spot. Rotating discs upon the face of the larger box provided for a radial series of light-spots, 5 mm apart, along any one of 360 diameters.

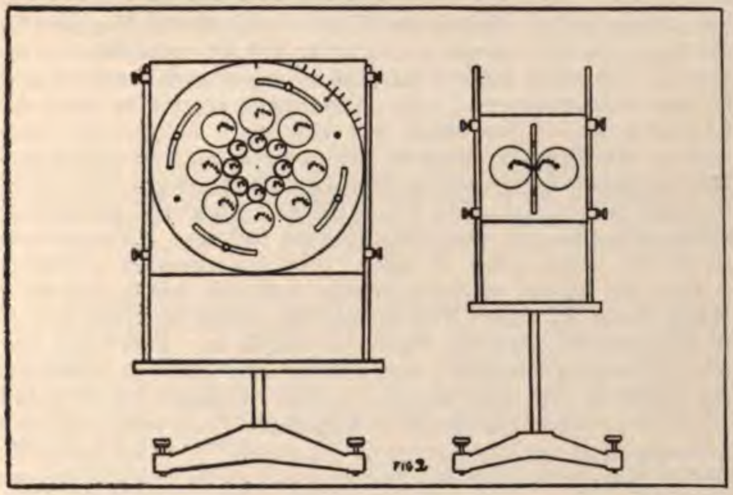


FIG 2

Method. C, D, and F served as observers. The observer was seated in a chair with his head fixed in a head-rest and his right eye covered with a black cloth. His left eye was at a distance of 1½ meters from the face of the small box, and the point of light at the intersection of the two lines of holes appeared opposite the fovea. This dot served as a fixation-point; the other openings were covered. Light was admitted through apertures successively nearer to the center of the face of the large light-box until those points were found which cast beams of light just within the area of the blind-spot. As a check upon the accuracy of the adjustment, the slide and the disc on the small box were so moved as to admit light through apertures 5 mm above or below, and at the right or the left of the original fixation-point. When either of these four adjacent spots was fixated, some of the points of light on the large box became visible. An extremely slight change in the position of the eye, then, brought the lights partially within the field of vision.

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[27] Perky (*op. cit.*, 437) employed a similar method. Our arrangement provides for the detection of considerably smaller movements of the eyes and also for a nice control of the position and intensity of the light-spots.

Before each experiment, the head-rest and the boxes were adjusted with reference to O's blind-spot, since a different arrangement was necessary for each observer. When O had taken his place in the dark room, he was given the following instructions: "You will be shown a series of four drawings, the experimenter naming each one as it appears.[28] Shortly after a 'ready' signal, a drawing will be exposed for 20 seconds. After a further interval, of 20 seconds, another will be exposed, and so on." The drawings were placed over the fixation-point of the small box and were illuminated from above. During this part of the experiment, the apparatus was concealed by a black screen with an opening (2" x 1½") through which the card was visible. After the perceptual series, the observer was instructed as follows: "You are to fixate the single point of light. The experimenter will pronounce the names of the drawings, and you are to call up a visual image of each. Try to hold the image constant, but, if it disappears, make no effort to recall it. Press the key when the image appears and release and press it whenever the lights at the side become visible. Release the key and say 'now' when the image disappears. Then give complete introspections for the period of the image." A kymographic record was made showing the appearance and disappearance of the lights as indicated by O. The experimenter timed each image with a stop-watch. During the last half of the series, each record was made for a period of 20 seconds, and the observer was required to recall the image if it disappeared during that time. Several other records were made as controls with each observer. Under the controls, O was told to fixate the point of light until the experimenter gave a signal to stop at the end of 20 seconds. Under these conditions, no attempt was made to arouse imagery. After each experimental period O's descriptions were transcribed from his dictation. When necessary, the experimenter questioned him with respect to (1) the kinds of conscious processes integrated with the total visual image, (2) associative processes, (3) the clearness of the image, and (4) the accompanying affective tone. O was then asked to sketch the object as perceived and also as imaged.

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[28] The following names were given to the figures: wij, bottle, ked, butterfly, fak, turtle, ged, star, pam, spool, tis, daisy, rel, key, bam, fish, hof, ring, lat, apple, zum, leaf, doj, cup, mej, knife, dak, pipe, kif, scissors.

Results. Although the drawings did not serve the purpose for which they were primarily intended, the results were significant in other respects. Most of the pictures devised for nonsense drawings either proved to be meaningful or were accompanied by meaningful associative processes. Only 7 of the 88 images had no meaning for the observer or signified simply "drawings-shown-a-few-minutes-ago." There was no appreciable correspondence between images of this sort and the presence or absence of ocular movement. The experiments did, however, supplement the results of the investigations which were mentioned in Section II with respect to the *size* and the accompanying *affective tone* of the image.

We have already cited a possible objection to the observer's reproduction of the image by a drawing. Since kinaesthetic and tactual sensations were involved in such a depiction, it was possible that the results were not exact representations of the images. But even if allowances are made for constant errors, the total results seem significant.

Since the drawings were 1½ meters away, we might expect the observer's representations to be slightly smaller than the originals. There was, however, a strong tendency, with two observers, to exaggeration in the image. No drawing was smaller than the figure by more than an inch, while 7 exaggerated it by 1-6 inches.

All observers reported a predominance of pleasantness over unpleasantness in the images (43:19). As a rule, however, the affective tone was connected with the free or impeded fulfillment of instructions, not with the character or the significance of the total images. Too often writers have assumed without warrant that coincident affective tone and image were inherently related.

The correspondence between *ocular movement, the sort of image,* and *the condition of attention* appears when we compare all the results obtained from the three observers. The number of images associated with ocular movements is shown in Table IX, and the duration of these movements with any image, in Table X. (I. stands for the period of the image; Co., for that of the control). Not only did D have fewer images accompanied by ocular movement than did C or F, but the number of movements with a given image was apt to be less in his case. There was also a noticeable difference in the general *character of the images* of the three observers.

TABLE IX

Observer	C.		D.		F.	
	I.	Co.	I.	Co.	I.	Co.
Ocular movement	29	4	7	4	28	3
No ocular movement	0	0	23	1	1	0

TABLE X

DURATION OF OCULAR MOVEMENT

Seconds		2	3	4	5	6	7	8	9	10	11	13	15	20
D	I.	..	..	1	..	..	2	1	..	..	..	..	..	3
	Co.	..	..	..	..	..	..	..	..	..	..	..	1	3
C	I.	1	5	4	4	2	8	2	..	2	..	..	..	1
	Co.	..	..	1	2	..	1	..	..	..	..	..	..	..
F	I.	1	..	3	1	2	7	1	2	4	1	2	..	4
	Co.	..	1	..	..	..	1	..	..	1	..	..	..	..

(1) The *size* of D's images, according to his drawings, varied but slightly from that of the diagrams, while both C's and F's images were exaggerated in approximately 68% of the cases.

(2) None of D's images were *incomplete,* but 11 of C's and 15 of F's lacked some or many details.

(3) Four of D's images, 8 of C's and 9 of F's revealed *inaccuracies* of detail.

(4) Three of C's images and 6 of F's were of *objects other than the diagrams.* Four times, in the case of F, these representations were incomplete. D was always able to arouse an image of the perceived drawing.

(5) D's total images never contained imaginal visual elements which were not represented in the corresponding perceptual objects. No processes from other sense departments were intimately integrated with the images. Such integration occurred with 5 of C's images. Four times the added elements were visual, and once they were kinaesthetic.

In 14 instances, F's total images were *elaborations of the perceptual object.* The added elements were visual in 3 cases, kinaesthetic in 8, tactual in 2, and organic in one.

(6) D always drew the same diagram as representing both the image and its corresponding percept. This was the case with only 11 of C's images and with none of F's.

Individual differences were noticeable, not only in the character of the image, but also in the *state of attention.*

(1) In the case of 22 of D's images, there were a few (from one to three) simple groups of associative processes, usually secondary (verbal). Twelve of C's images and 19 of F's either lacked such processes or were accompanied by a great many. Attention was apt to fluctuate when associative processes were so numerous as to act as distractions or when the absence of these processes made the images uninteresting.

(2) Further differences existed with respect to the degree of clearness and its fluctuations. Over half of D's images were very clear and steady. Twenty of C's images were very clear, but 19 of them fluctuated. Moderate clearness was characteristic of 15 of F's images, and fluctuations occurred in 24 cases.

(3) D's attention was less frequently secondary (or active) than was C's or F's. D reported a conflict between groups of processes, or attention with effort, 6 times, C 11 times, and F in 9 cases.

In brief, *fixation was usually steady* when an image was very *clear* and *did not fluctuate,* when it was accompanied by *few associative processes,* and when *attention was without effort.* Under other conditions ocular movement tended to occur.

The dissimilarities among the images of a single observer are associated with ocular movement in the same way as are variations in all the images of different observers.

In the six cases in which D's attention was secondary, fixation was unsteady. During the course of 4 other images the eyes moved, although attention was without effort, but in all of these instances there were many associative processes or irrelevant ideas. The one image which was noticeably vaguer than any of the others was also accompanied by ocular movement. All of C's images were accompanied by movements of the eyes, but the one image with which there was only a single movement in 20 seconds was very clear and constant, and attention was without effort. One of F's images was accompanied by no ocular movement. Attention was without effort, and the image was clear and constant. With 4 of his other images, there was only one movement in 20 seconds. In all of these cases, attention was without effort. Two images were very clear, and the other 2 were moderately clear, while the images remained constant or fluctuated but slightly.

If we turn to the kymographic records of the control-experiments, we see that they are in accord with the results already mentioned. Ocular movement frequently occurred during the controls, and, when it did, the general conditions of consciousness closely resembled those which were characteristic of the images accompanied by ocular movement.

To sum up, we may say that there is usually no movement of the eyes with images which are fairly faithful representations of the corresponding perceptual objects; while ocular movement tends to occur with images which differ from the perceptual objects in size, and in completeness and accuracy of detail, and, in some cases, when processes from other sense departments are integrated with the images. Moreover, fixation is apt to be steady when there are few associative processes (but not so many as to distract attention from the image and not so few as to allow attention to wander), when the image is clear and stable, and when attention is without effort. When these conditions do not exist, ocular movements tend to occur.

The results of Experiment 3 are in general accord with those of a preliminary series in which C alone served as observer. The apparatus was a rough wood and cardboard model like the metal boxes in all essential details. A series of words was used to arouse images. O was required to time the images with a stop-watch and to press a recording key whenever the points of light became visible. During the course of 15 of the images, a beam of light was thrown upon the cornea of O's eye as in Experiment 1. Unfortunately, the light had to be of such a low intensity that the experimenter's observations of ocular movement are not reliable. The experimental conditions, especially the presence of the beam of light, distracted the observer. For this reason, the value of the results might be questioned if they were not in agreement with those of the other series.

Records of 22 images were obtained. Two of these were U-images, 13 were G-images, 7 were F-images. Of the F-images, 6 had somatic reference, 5 had a recognized scenic background, and 3 had fairly many associations. Of the G-images, none had a scenic background, and 11 had few or no associations. Ocular movements were reported by the observer with 5 (71%) of the F-images and with 7 (54%) of the G-images.

As regards attention to the image, there was no ocular movement in the one case in which the image was very clear and constant, or with another image which showed a gradual decrease in clearness. Ocular movement occurred 1, 2, or 3 times in 5 of the 10 cases in which the image was vague but exhibited no appreciable shifts in clearness, and in 8 of the 10 cases in which there were marked fluctuations in the clearness of the image. There was evidently a degree of correspondence between the frequency of ocular movement and the kind of image, and a rather marked relation of ocular movement to attention.

Experiment 4

Problem and method. While Experiment 3 was in progress, it occurred to the writer that, in spite of the apparent accuracy of the apparatus, the observer might sometimes be unaware of the spots of light even when the eyes moved. If O's attention were focused upon the visual image, there was a possibility that the light might fall upon a sensitive portion of the retina without touching off a report. To test this hypothesis, we combined the methods of Exps. 2 and 3. With the left eye, the observer fixated the single point of light as in the preceding investigation. The right eye was closed, and the tambour used in Exp. 2 was adjusted against the lid. In general, the procedure was the same as in Exp. 3, except that, instead of exposing the drawings in a perceptual series and later arousing the corresponding images, a number of words were used as in Experiment 2. C, D, and F again served as observers. As a preliminary control, before each record was made, the observer intentionally moved his eyes. In all of these cases the movement was recorded and the dots of light became visible. Each record was taken for a period of 20 seconds, as in the latter part of Exp. 3.

Results. Six records were made with D as observer, 5 with F, and 8 with C. In D's case, no ocular movement was detected by either method. The dots were never visible during the 20 seconds of fixation, and the record made by the pneumatic system showed no fluctuation. During the course of 4 of F's 5 images, dots became visible, but the other recording line did not waver. Very slight ocular movements evidently sufficed to bring the dots within the field of vision, but were not large enough to affect the tambour. One of C's records indicated that the dots became visible 5 times while there were no corresponding fluctuations in the other line. In the other 7 cases, ocular movement was indicated by both lines. There was a general conformity between the two records, although in some cases the record made by the pneumatic system indicated more fluctuations than the other line.

There are several possible explanations of these apparent inconsistencies. (1) C sometimes saw the points of light 8 or 10 times in 20 seconds. It is very likely that she did not react quickly enough to record all the changes. (2) A twitching of the right lid might have changed the one record, although the eyes themselves remained steady, so that no dots appeared. (3) Where the excursion of the eyes was fairly wide, a fluctuation in the pneumatic record might have occurred when the projection of the cornea crossed the bulge of the tambour, and again when the eye resumed its original position, although the dots were visible but once. While one record was being made, the electric current was accidentally reduced, and the points of light became very faint. It was probably due to this fact that the dots were visible less often than in the previous periods.

The results of this experiment seem to indicate that the suggested hypothesis is incorrect. If O's eyes had moved while he failed to notice the points of light, the pneumatic record would have fluctuated, while the other was unaffected. But this was not the case. When movement was recorded by only one line, it was the record made by O that showed fluctuations. The pneumatic system seemed the less accurate of the two. When slight movements of the eyes occurred, the observer noticed the dots, but the tambour was apparently not affected, while larger movements were recorded by both systems.

From the results of Exp. 4 we infer that the perception of the dots of light during the experimental period indicated actual movement of the eyes. The results of the short trial preliminary to Exp. 3 revealed the same correspondence between F-images and G-images which was discovered in Exp. 1. Ocular movement usually occurred with the former images, but not with the latter. In Exp. 3, we found that the amount of resemblance among imaged objects and the corresponding perceived objects seemed to be connected with ocular movement. As has already been suggested, this correspondence may reasonably be referred to an association of ocular movements with certain conditions of attention. Both Exp. 3 and the preliminary trial confirm the correspondence of ocular movements with the degree of clearness and fluctuations of attention which was the most significant discovery in Exp. 2. In Exps. 1 and 2, the arousal of a particular image was not required, and the experimental conditions did not seem to impose any tax upon the observers' ability to hold the attention steady. In Exp. 3, however, there occurred, as a result of the exacting instructions, numerous cases of secondary attention. There proved to be a positive correspondence between this sort of attention and ocular movements.

Were we to attempt an explanation of the correspondence which we have discovered between different sorts of image, attention, and ocular movement, we should turn naturally to the facts of perception. A series of special experiments would be necessary fully to substantiate a transfer of general motor attitude or of ocular movement from perception to visual imagery. Our results suggest, however, that there are several possible likenesses among perceptions and images in respect to movements of the eyes.

(1) When an extended perceptual object close at hand has many details, we must, in order to gain an accurate impression of it, glance now at one portion, now at another. In a similar way, we have frequently discovered ocular movement and shifts of attention where an imaged object is rich in detail or is accompanied by numerous associative processes (as was the case with many of the F-images and with some of the images of drawn figures in Exp. 3).

(2) When a perceptual object has very few details, we are apt to grasp its significance at a glance and then to turn our gaze and our attention to something else. A similar tendency was manifested, in our experiments, in the case of some uninterestingly simple images.

(3) In perception, when an object lacks cognitive clearness, our eyes are often focused first on one part, then on another, in order to gain a more definite impression. Such waverings of the glance were also characteristic of images.

(4) Under normal conditions, when we attend successively to different aspects of a perceptual object, our glance almost invariably follows the direction of our attention. We have already given instances of the agreement between fluctuations of clearness and shifts of fixation in imagery.

(5) Finally, when two rival objects claim the perceptual attention, the glance is apt to waver between them. Our results showed that secondary attention to imagery also involved ocular movements. All of these resemblances seem to point in the same direction. In the absence of contradictory evidence we may, then, reasonably suppose that the relations existing between the character of the object, attention, and ocular movements in perception are transferred to analogous cases of imagery. Such an hypothesis is in harmony with the current view of the physiological functions underlying perception and imagery. When an object formerly perceived is imaged there occurs, presumably, through central excitation, a reinstatement of the approximate physiological conditions of perception. The imaged object presents much the same color, shape, size, and details as does the perceptual object. There might easily be a similar reinstatement of muscular innervations. In fact, the recurrence in imagery of the movements of the eyes involved in perception is a more probable phenomenon than is the accurate repetition of the visual details of the perceived object, since ocular reactions which are alike (at least in their gross characteristics) occur with a large number of perceptions, while a comparatively small number of objects present a close resemblance in their qualitative aspects. That is to say, there may be more likelihood of the integration of generic muscular reactions in imagery than of the formation of generic visual images.

IV. Summary and Conclusions

The results upon which we base our conclusions were obtained from six trained observers, four of whom served in two or more of the four principal experiments. The ocular movement occurring in connection with visual images was recorded in the various investigations by three different methods. There was a general agreement in the results whenever two or more experiments involved the same problems. Although individual differences were sometimes conspicuous, they were of such a sort as to substantiate our conclusions, not to invalidate them.

Agreement between our results and those of other investigators is as close as we could reasonably expect in view of the differences which obtain in instruction and method, and, especially, of the differences in the criteria used for classifying images. (See Table XI). The "memorial image" of both Ogden and Perky should probably be considered as a sub-class of our F-images. Perky's images of imagination we should generally have classed—so far as we can tell from her descriptions—as G-images; Ogden's images of imagination,[29] as U-images. We found that our G-images (Perky's imaginations)[30] and our U-images (Ogden's imaginations) were usually unlike in most of their characteristics. It is not strange, then, that there should be considerable disagreement between Perky's and Ogden's results when these two classes are compared. Koffka's general images would coincide in part with the G-images of our classification, while his individual images would include both our F and U classes.

TABLE XI

	Specific Reference		Generic Reference
Clark	F-Images	U-Images	G-Images
Perky	Memory (part. and pers. ref.)		Imagination
Ogden	Memory (fam.?)	Imagination	
Koffka	Individual (degrees of ind'ing and loc'ing)	Individual	General (in part)

1. Five of the characteristics of images which we have examined were also considered in much the same way by

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[29] Ogden appears to have left his observers free to apply their own criteria to their images: he says, "The criteria which seemed to be employed primarily by our observers were those of 'familiarity' and 'unfamiliarity'" (p. 384).

[30] Perky writes of "generic images" and of "habitual imaginations" (pp. 440, 447) in contrast to "particular images."

Perky, Martin, and Ogden. The following summary gives a comparison of the results.

(a) There was a general agreement in respect to *associative processes,* which frequently accompanied the memorial images of Perky,[31] Martin,[32] and Ogden,[33] and were found with our F-images, and also with Ogden's images of imagination and our corresponding U-images. With Perky's[34] and Martin's[35] images of imagination and with our corresponding G-images these accompanying processes occurred less often.

(b) Three of the four investigations indicated similarities of *clearness* and of *detail.* The memorial images of Ogden[36] and of Martin,[37] and our F-images, were apt to be distinct, not "scrappy" and "filmy," as were Perky's. Instead of being "substantial" and "complete," as were Perky's images of imagination,[38] our corresponding G-images often presented few and vague details. Many of Ogden's images of imagination[39] and of our U-images were detailed and clear.

(c) The results of Ogden[40] and of Martin[41] also agree with ours in failing to show any correspondence between the kind of image and the presence or absence of *color.* Perky gives no statistical summary, but describes the images of memory as "colorless etchings," and those of imagination as "oftentimes, on the contrary, very highly colored."[42] Her statements, however, appear to be contradictory, for she also states that the images of memory "retained their daylight character," while those of imagination took the illumination of their surroundings.[43] This statement might be interpreted as meaning that it was the memories that were colored and the imaginations that had no color of their own.

(d) The results of Ogden's investigation and of our own do not agree with Perky's with respect to *affective tone.* Ogden[44] found no evidence of Perky's memorial "mood of recognition" or imaginative "mood of surprise." In our investigation, the images (especially in the F class) were frequently pleasant, but an explicit feeling of familiarity was not always found with F-images, and a feeling of strangeness or surprise occurred with only two U-images.

(e) As regards *stability,* there is no correspondence of results. Perky's memorial images were described as "scrappy and fleeting,"[45] her images of imagination as "substantial."[46]

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[31] Perky, p. 451.
[32] Martin, p. 408, 412.
[33] Ogden, p. 405.
[34] Perky, p. 450.
[35] Martin, p. 408, 412.
[36] Ogden, p. 398.
[37] Martin, p. 400.
[38] Perky, p. 451.
[39] Ogden, p. 398.
[40] Ogden, p. 396.
[41] Martin, p. 405.
[42] Perky, p. 447.
[43] Perky, p. 446.
[44] Ogden, p. 398.
[45] Perky, p. 447.
[46] Perky, p. 451.

Martin's images of memory were usually, though not always, more stable than those of imagination;[47] and Ogden found about the same number of stable images of his two kinds.[48] Our investigation revealed no significant correspondence between the kind of image and its stability. Where a comparison is possible, the total results of Ogden and Martin agree, in general, with those of the writer, while all of these three investigations give evidence which is contradictory to that of Perky.

2. As regards *ocular movements,* our results agree with Perky's in so far as these movements usually occurred with her images of memory and frequently with our larger class of F-images, but were not, as a rule, reported with her images of "imagination" or with our corresponding G-images. The frequent occurrence of movement with our U-images, however, suggests that no essential relation obtains between images of "memory," as contrasted with other forms or functions of imagery, and these kinaesthetic processes. That is to say, the conditions of ocular movement with imagery seem to be *general* conditions,—not the conditions which distinguish two imaginal functions, memorial and imaginative. Indeed, it is doubtful whether it is feasible and proper to attempt to oppose these two functions at the level of the total image. Our own analyses, as well as those of Koffka, suggest, instead, a very large number of functional gradations for the simpler imaginal complexes;—gradations of specifying, of individualizing, of generalizing, of symbolizing, and of reference to the observer, to objects, to times, to places, and to contexts. The alleged functional distinctions based upon "particularity" and "personal reference" are gross, and, moreover, we are not persuaded by the facts that they are correlated with such differences in process and integration as temporal course, clearness, substantialness, and affective and organic accompaniments. As for the presence and absence of ocular movements under imagery, we are not certain that familiarity, "the mood of memory" (Perky, p. 451), is essentially connected with this special kinaesthesis; while we have failed to discover by introspection an antithetical or contrasted mood of unfamiliarity or "novelty" to mark the absence of such movements. These specialized movements we are inclined to account for in another way. They seem to us to be symptomatic of those general central conditions which underlie *determination* and which fix the state and the configuration of conscious-

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[47] Martin, p. 407. [48] Ogden, p. 404.

ness,—*i. e.*, fix attention and associative completion.[49] Whenever these general conditions approximate or tend to reproduce the usual psychophysical accompaniments of visual perception, then the ocular movements also tend to occur. Visual perceptions normally flow in trains, passing from feature to feature of the object and likewise from object to object. The kaleidoscopic change of clear processes bears with it a succession of ocular movements. We believe that whenever this diffusive and exploratory kind of visual attention occurs, whether or not the organism is immediately affected by a visual stimulus, there normally appear, save under some special inhibition, the small movements of the eyes which have now been detected in many experiments set for the study of imagery.

If our view is correct, then the presence and frequency of ocular movements would have no immediate or intrinsic connection either with familiarity or with "personal reference" and "particularity;" though it might well be true that those images which make most explicit reference to the observer and to his past experiences also most frequently fulfill the general conditions just referred to.[50] It follows that isolated, detached, unset and un-detailed objects, such as appeared in our G-images, would least reinstate the usual conditions of perception; but it does not follow that these objects furnish the materials of "imagination." We are thus led to connect ocular movements more directly with general central and conscious conditions than with special functional differences displayed by the image.

In so far as it is permissible to base generalizations upon our experimental results, we feel justified in drawing the following conclusions:

1. If visual images are divided into three groups upon the basis of the presence or the absence of (1) specific reference and (2) familiarity of the object, certain other functional characteristics are found to be rather closely associated with a particular group or with groups of images. Pure types, however, are comparatively rare, and intermediate forms are relatively numerous.

2. A correspondence discoverable between kind of image

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[49] Perky found that attention wandered in memory but was narrowly focused in imagination (p. 449).

[50] As regards audition, olfaction, and the other senses, the facts are not sufficient to warrant general conclusions. Remembering the history of the doctrine of "fluctuations of attention," the author is suspicious of any general functional difference which does not take into account the bearing of sensory adjustments.

and amount of ocular movement is probably to be referred to certain general conditions of attention.

3. Changes in the clearness of the image correspond, to an appreciable extent, to movements of the eyes.

4. Ocular movement is more likely to occur in secondary attention than in primary or derived primary attention.

5. Characteristic ocular movements, and possibly general motor attitudes, seem to be transferred from visual perceptions to visual imagery.

RECONSTRUCTIVE RECALL*

By GARRY C. MYERS and CAROLINE E. MYERS, Brooklyn

It is common observation that if one fails in an attempt to recall a bit of one time familiar experience, later one may recall numerous remnants of it, and may reproduce the greater part or even all of that experience.

With a purpose to estimate how far one can recall what seems to have been forgotten, and to determine some of the conditions under which the forgotten can be reconstructed the following study has been undertaken.

The six subjects were instructed to try to reconstruct from memory, one or more selections, which they once readily could quote, but which in the meantime, they almost completely had forgotten. They were told to make their attempts at recall at regular intervals or at the most convenient time. They were asked to introspect and were urged especially to note as far as possible the conditions under which the recalls were made. Some worked at several selections over the same period of time. Others worked upon only one at a time, and, when they had given that up in despair they turned to another selection. The period of time over which the subjects worked varied from a few weeks to several months.

One subject recalled at regular intervals; she carefully noted her time for recall which was generally ten minutes. The other five were irregular in intervals, and length of recall times, and one of these merely indicated the order of the recalls without regard to the time when they were made. Two subjects, G. M. and C. M., made several recalls in a day and they spent from 5 minutes to an hour at recalls. Only three indicated the interval between learning and recall. There could hardly be an absolute certainty as to the absence of recalls during these intervals. One subject, G. W., was certain she had no recalls of one selection while she stated she had many recalls of another selection, during the interval.

Since not all the materials readily admit of statistical analysis some typical records will be quoted and some of their prominent characteristics will be pointed out.

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* Reported, in brief, at the November (1915) York Branch of the American Psychological

The following from Lincoln's Gettysburg Address was given by Miss T.

'Four score and twenty? years ago our fathers brought forth on this continent a new nation conceived in liberty and dedicated to the proposition that all men are created equal. We are met on a great battle field of that nation. We have come to dedicate a portion of that field as a final resting place for those who here gave their lives that that nation might live.'

Note—"I also remembered part of the last phrase—'of the people, by the people and for the people.'

The question mark after *twenty* means that my common sense told me it could not be *twenty*, altho I wrote that number and could not remember the right one. I started the test January 26, 1915, and took twenty minutes every morning for the experiment."

January 27th. "I remember that somewhere near the middle of the address there were the words 'above our power;' that some people said 'our poor power,' but I knew I had been told that 'poor' should not be there."

January 28 to January 31 "I added no new words."

February 1. "While playing the piano and being seemingly very interested in a new piece I was trying, I suddenly found myself saying, 'The world will little note nor long remember what we say here but it can never forget what they did here.'"

February 2. "When I opened to the first page of a book I just got from the library I saw written 'To J. R. D. I dedicate this book.' The word *dedicate* seemed to remain with me and even at dinner I found myself saying it. Then when I started to think of the selection I remembered that part of it went thus: 'But (?) —— we cannot dedicate we cannot consecrate we cannot —— this ground.'"

February 3. "In the morning when my mind was on the experiment and I deliberately tried to remember more of it I got only the very last few words, 'shall not perish from the earth.'"

February 4, 5, 6. "No new words. On the 5th in talking to a friend H. B. about another individual X, H. B. said she thought X should carry on the noble work which her mother (X's mother) had been striving so hard to finish. I suddenly remembered that that idea was expressed in the Gettysburg Address somewhat this way: 'It is for us the living to carry on the unfinished work which those who fought here have —— so —— advanced."

February 7. "In the evening I sat down and tried to say the address right off and thus got some new words. After a few attempts, probably 6 or 7, I found I had picked up two new words. In the sentence 'But we cannot consecrate we cannot dedicate we cannot hallow this ground. *Hallow* was a new word. I went on and finally got a good deal more as follows: 'The brave men living and dead, who struggled here have consecrated it far above our power to add or detract.' The words 'above our power to add' I had had before.

"I also got the rest of the sentence of February 5th as follows: 'It is for us the living rather to be dedicated here to the unfinished work which they who fought here have thus far so nobly advanced.'"

February 8. "In the assembly hall to my regret the pupils said the Gettysburg Address. ——.

"My test of remembering this speech brought up many facts, ideas and people that I had forgotten since my elementary school days."

Subject G. W., after an interval of nine years, recalled, in 16 trials over 13 days, 65 per cent of the "Wreck of the Hesperus." Of this, 44 per cent was added after the first recall. Variations and confusions were very pronounced. "For a long time," she wrote, "the first verse seemed to be,

> 'It was the schooner Hesperus,
> That had sailed the wintry sea!'
> 'By the long gray beard and glittering eyes,
> Now wherefor stoppest thou me?'

"These last two lines from the Ancient Mariner would persist in preventing my progress."

Of her fifth and sixth trials she wrote: "Badly mixed up with Ancient Mariner."

Confusions within the poem were frequent. For example, of the stanza,

> "He wrapped her warm in his seaman's coat,
> Against the stinging blast;
> He cut a rope from a broken spar,
> And bound her to the mast."

she recalled in the third trial, *rough* for *seaman's; lashed* for *bound* and omitted *stinging*, and *broken spar*. She placed the fourth line for the second. Five days later she got *stinging blast*, but could not place it until the final reconstruction, after six more days. This delay in proper placing till the final recall was quite frequent for G. W., as well as for some of the other subjects. In her final recall she interchanged the third and fourth lines of stanzas XI and XII, respectively, whereas in her first recall they were correctly placed. Likewise in her third trial she crossed out a half line of a stanza and substituted a wrong phrase, but corrected it in the final recall. Both stanzas XV and XXII have as rhyming words, *snow* and *Norman's woe*. When, however, she recalled *driven snow* where she should have *carded wool*, in stanza XVIII, she immediately gave *Norman's woe* as the rhyming word. Space does not admit of her many other substitutions, confusions, doubtful factors, and omissions.

The same subject added 33 per cent of the Psalm of Life in 8 subsequent recalls. In her first recall of stanza IV she got line 1 and 4, but could get none of line 2. She thought however, that line 3 ended in *heart is beating*. This really is the gist of lines 2 and 3:

> "And our hearts though stout and brave,
> Still like muffled drums are beating."

On the evening of the same day she recalled the whole stanza correctly.

Subject G. M. added 44 per cent in four subsequent recalls of the same poem. In his fourth trial he recalled: "Art is long and time is fleeting" and "funeral marches to the grave," but did not know that they belonged to the same stanza. Later in the same day he got:

> "—— —— —— —— drum beat
> Funeral marches to the grave."

Then he recalled:

> "Art is long and time is fleeting,
> And our hearts though stout and

"When this was written," he noted, "I got *drum beat* for the end of the third line; then *Still like* came up, then *drum beating*, then came *muffled drums* and all at once,

> "Still like muffled drums are beating
> Funeral marches to the grave."

For him the following lines from "The Present Crisis" frequently obtruded but were seldom accepted as true:

> "We ourselves would pilgrims be
> Launch our Mayflower and steer boldly
> In the desperate winter sea."

WOODMAN, SPARE THAT TREE

Recalled by Miss M. S.

No. 1. I

1st Trial. Woodman, spare that tree! Touch not a single bough;
In youth it sheltered me and I'll protect it now;
2d Trial. 'Twas my forefather's hand that placed it near his cot
There, woodman, let it stand, thy axe shall harm it not!

Note—"In the 2d trial the last line of the stanza came to me before the third line. I kept repeating the first two lines over and over again trying to remember some more. I remembered a movement of my hand that I had been taught when I said, 'There,' this gave me a cue to the last line. I tried to find a word that rhymed with 'not.' When I thought of 'cot' I remembered the rest of the line."

No. 2. II

8th Trial. That old familiar tree, its glory and renown
7th Trial. Are spread o'er land and sea, and would'st thou hew it down?
3rd Trial. Woodman forbear thy stroke! Cut not its earth-bound ties;
5th and
6th Trial. Oh! spare that aged oak now towering to the skies.

Note—"Some one asked me what I was trying to remember; when I told them they said 'That old familiar thing; anyone could remember that,' then I got the first line of the stanza."

"I was listening to a speaker who said 'The blood of our best men is spread o'er land and sea,' and involuntarily I supplied, 'and would'st thou hew it down.' Try as I would I could get no more."

"I recalled that when I was learning the poem I had been told to say it as though I were entreating some one then I remembered the third line but could not place it in the poem."

"In this trial a friend who had heard me reciting some of the poem and was impatient for me to join her shouted, 'For goodness sake spare many and come on,' then I thought of the first half of line four."

"When I repeated the poem I remembered that—'Oh! spare that aged oak' came after line 3, stanza II. When I said the two together I got the rest of the fourth line."

No. 3. III

8th Trial. When but an idle boy I sought its grateful shad-
10th Trial. In all their gushing joy, here too my sisters play

4th Trial. My mother kissed me here; my father pressed my hand,
1st Trial. Forgive this foolish tear, but let that old oak stand!

Note—In the 10th trial "I finished reading a letter which was remarked as gushing. When I sat down to recall some of the poem I remembered 'here too his sisters played in all their gushing joy.' When I tried to fit it into the verse I found that I had remembered it backwards."

In the 4th trial "I had been thinking for a long time but got nothing new. At last I read what I had written and I began wondering what it was that made him cry in line 4 verse III. Then I remembered that it was something about his mother and father. I repeated to myself 'My mother and father,' 'My mother and father' until I thought of the line."

When the subject got the 4th line of this verse she could not locate it in the poem.

No. 4. IV

11th and
12th Trial. My heart-strings round thee cling, close as the bark old friend!
13th Trial. Here shall the wild bird sing and still thy branches bend.
Old tree, the storm thou'lt brave, and woodman leave the spot;
While I've hand to save thy axe shall harm it not.

Note—In the 11th trial "I had just finished reading 'Apollo and Daphne'. Daphne turns into a tree and her garments become her bark. This made me get the last half of line 1."

In the 13th trial "While I was telling a story to a group of children one of them annoyed me so much that I finally said 'Jack, leave the room.' I said it so dramatically that we all laughed and when I was thinking of it later it reminded me of the way I had said 'Woodman, leave the spot.' Then the verse came to my mind."

In the 9th trial the subject got nothing new.

She did not indicate the date of each trial.

One can see that, although the lines came up in the most perverted order they, as well as the stanzas, finally found their proper places.

QUOTATIONS FROM "SNOW BOUND"

Recalled by G. M. after an interval of ten years.

"1. The sun that brief December day
2. Rose cheerless over hills of gray,
3. And, darkly circled, gave at noon
4. A sadder light than waning moon,
5. Slow tracing down the thickening sky
6. Its mute and ominous prophecy,
7. A portent seeming less than threat,
8. It sank from sight before it set.
9. A chill no coat, however stout,
10. Of homespun stuff could quite shut out,
11. A hard, dull bitterness of cold
12. That checked, mid-vein, the circling race
13. Of life blood in the sharpened face,
14. The coming of the snow-storm told.
15. The wind blew east; we heard the roar
16. Of Ocean on his wintry shore,
17. And felt the strong pulse throbbing there
eat with low rhythm our inland air."—The original.

Notes—On January 12th the subject recalled the first eight lines correctly except that he substituted "A" for "Its" in line 6. At this first recall he also got lines 11, 15, 16, 17, 18, and two lines from Quotation II.

Lines 9 and 10 were recalled correctly on January 16th but it was not until February 1st that they were marked "Sure."

Line 11.
Jan. 12th. A dull chill bitterness of cold
Jan. 16th. A dark dull bitterness of cold
Feb. 1st. A hard dull bitterness of cold (correct)

Line 12.
Jan. 16th. Thought line ended in "race".
Later. That checked mid-life the —— face,
Later. That checked mid-vein the encircling race,

Line 13.
Jan. 16th. Of life blood in the hardened face,
Jan. 17th. Of life blood in the darkened face,

Line 14. Omitted.

Line 15.
Jan. 12th. Correct.
Feb. 1st. Placed it after line 13.

Lines 16 and 17.
Jan. 12th. Correct.

Line 18.
Jan. 12th. Beat with low rhythm the wintry air.
Jan. 15th. Beat with low rhythm our inland air.

Quotation II.

"1. Unwarned by any sunset light
2. The gray day darkened into night,
3. A night made hoary with the swarm,
4. And whirl-dance of the blinding storm;
5. As zigzag wavering to and fro
6. Crossed and recrossed the winged snow;
7. And ere the early bedtime came
8. The white drift piled the window-frame,
9. And through the glass the clothes-line posts
10. Looked in like tall and sheeted ghosts."—The original.

Line 1.
Jan. 23rd. And ere the early candle light
or
Long ere the time for candle light
Jan. 26th. Unwarned by sunset's mellow light
Jan. 27th. And ere the time for —— ——
Feb. 1st. And ere the time for candle light
Feb. 7th. Line above was marked correct.

Line 2.
Jan. 16th. The twilight softened into night,
Jan. 26th. The gray day softened into night, (Marked "sure")
Feb. 1st. Same as above. *Darkened* had been crossed out and *softened* written in.
Feb. 7th. The gray day darkened into night, (Correct)

Line 3.
Jan. 27th. A night made hoary with the blast
Feb. 1st. A night made (wretched with the flow)—doubtful
Later. A night made bitter by the blasts

Feb. 7th. A night made bitter by the frost
Later. A night made dreadful with the storms
Feb. 11th. A night made blinding by the swarm (Marked "sure")

Line 4.
Jan. 27th. Of snow flakes driving thick and fast;
Feb. 1st. Of snow flakes and the driving snow;
Later. Of snow flakes and the rising masts;
Feb. 7th. And ghost like with the rising forms;
Feb. 11th. Of snow flake and the blinding storm;

Note—Grouping lines 3 and 4 of the same date the vain attempt to reconstruct by rhyme is manifest.

Lines 5 and 6.
Jan. 14th. Correct.

Line 7.
Jan. 12th. And ere the early bed-time (or evening) came
Jan. 19th. And when the early evening came
Later. And ere the early bedtime came

Line 8.
Jan. 12th. The snow had piled the window frame
Jan. 13th. The snow drifts piled the window frame

Line 9.
Jan. 12th. Appeared the covered posts
Jan. 13th. And from below the covered posts
Jan. 14th. And from below the haunted (or mounted) posts
Jan. 19th. —— —— —— —— —— posts
Jan. 23rd. And from without the mounted posts
Jan. 27th. And from without the snowclad posts
Feb. 5th. And from without the vaunted posts
Feb. 12th. And from without the clothes-line posts

Line 10.
Jan. 12th. —— —— —— —— haunting ghosts
Jan. 13th. Looked in like tall and sheeted ghosts.

THE PAINTER OF SEVILLE BY SUSAN WILSON.

Recalled by C. M.

Subject C. M. over a period of nine months reconstructed 132 lines of a poem of 183 lines which she had not recalled for about 5 years. Although her records show a gain of 101 lines in the 10 subsequent recalls over the first recall, space does not permit a complete analysis of her data. In her final recall only 15 words and phrases were wrongly used. There is no absolute certainty, however, that the selection had not been learned with some of these wrong words. While the substitutes are rarely synonyms almost all of them have obvious likeness in meaning to the correct words and none make a break in the story's thread. Some of them were *gleamed* for *beamed*, *proving how fruitless is the teacher's power* for *to prove how vain must be the teacher's care*, *fair* for *rich*, *failed to tell* for *did not tell*, *whispered* for *murmured*, *gleamed* for *shone*, *enrapt* for *absorbed*, *humble* for *trembling*.

The variations in words and phrases in successive recalls are worthy of note.

1. Only a *boy* the *lad* had seen Feb. 8th became
   Almost a *child* the *lad* had seen Feb. 9th (a) became
   Almost a *child* the *boy* had seen Feb. 9th (b) (Correct) became
   Almost a *child* the *lad* had seen Final. Nov. 1st.
2. *Gleaming* Feb. 8th became *glowing* Final (Correct).

3. *Shed* or *spread* Feb. 9th became *shed* (correct) Final.
4. *Softly* Feb. 9th became *mildly* (correct) became *kindly*. Final.
5. *Watch* Feb. 9th became *guard* (correct) May 4th and Final.
6. *And answered* March 18th became *and whispered* July 27th became *and said "I swear it"* Nov. 1st and Final.
7. *Shone* July 28th (a) became *glowed* July 28th (b) became *gleamed* (incorrect) July 29th and Final. *Beamed* was correct.
8. *Morning sunlight* or *early sunlight* July 28th became *early sunlight* (correct) Final.
9. *Gleaned* or *fell* July 29th became *gleamed* (correct) Final.
10. *Choice pupils* July 29th (a) became *young aspirants* July 29th (b). *Young aspirants* (correct) Final.
11. *Showing* or *proving* or *to prove* (correct) July 29th became *proving* Final.
12. *Teacher's part* July 29th (a) became *teacher's task* July 29th (b) became *teacher's power* (incorrect) Final. *Teacher's care* was correct.
13. *Long got knowledge* July 29th (a) became *unbought knowledge* (correct) July 29th (b) and final.
14. *Painter* or *Murillo* Feb. 9th became *Murillo* (correct) Final.
15. *Toiled* Feb. 13th (a) became *worked* Feb. 13 (b) became *joyed* Feb. 13th (c) (correct) and final.
16. *So be thee from this day* Feb. 13th became *So be thee from this time* (incorrect) Final. *So be thee mine by other bonds* was correct.
17. *Stroke* Feb. 15th became *touch* (correct) Final.
18. *Pressed* Feb. 13th became *clasped* March 18th became *pressed* (correct) Final.

One may conclude from the above that the "corrections" are practically always made in the right direction and that the errors are in words and not in content. In her introspections the subject stated: "I always got the story before the words." "When the lines 'But constant to his purpose still. He toiled to see his pupil gain' were first recalled I gave *worked* and then *toiled* for *joyed* but realized that it was incorrect. I knew that there was the thought that Murillo found pleasure in helping Sebastian. This thought was not expressed in *toiled* or *worked* but came with *joyed*."

There are two sections of the poem lines 1 to 18 and 64 to 72 which have similar beginnings:

"'Twas morning in Seville; and brightly beamed
The early sunlight in one chamber there;
Showing, where'er its glowing radiance gleamed,
Rich varied beauty."

and

"'Twas midnight in Seville; and faintly shone
From one small lamp a dim uncertain ray,
Within Murillo's study."

Until the 18th recall the subject gave lines 64 to 72 as the beginning of the poem. It was at this time that lines 1 to 4 were recalled.

In connection with lines:

"Nor dare to close your eyes in sleep
If on the morrow morn you fail
To answer what I ask
The last shall force you—do you hear?
Hence to your daily task."

The subject confused a phrase of another poem—"The Vision", "Thy faithfullness to show." "This phrase came up every time I got here and disturbed my going farther altho I knew it did not belong here."

In the final recall when the parts from the previous recalls were put together 12.5 lines were added and distributed as follows: 1, 1, 1, 8, 1.5. The subject did not realize, however, that these were being added. This fact emphasizes the interdependence of associated units, and is a vivid example of now-conscious memory.

Usually the recalls were made in groups of several lines. One rhyming word brought up another line. One line or part of one line rarely came up alone. She recalled at very irregular intervals. When February 14th she succeeded in dragging herself to do her best she made several recalls during the evening. She became obsessed with the task and was disturbed on lying down to sleep, by incompleted parts fleeting through her mind. She awoke in the night recalling some new parts. The whole day following she was distressed by the feeling of failure to recall what she was sure she could recall. During the day and previous evening she added in all 25 lines. Probably several hundred attempts were made with one part or another. The experience was so unpleasant that the subject ignored it and refused to write any recalls and tried to forget all about it so that although 4 lines came up 2 days later no more recalls, nor attempts at recall, were made until July 29th. She again evaded the task until November 1st. She noted then that if she could only get a few connecting words she could finish it. After reading the original to compare it with her final recall she repeated the entire poem in perfect order with only 9 words wrongly used.

In the table appended the numbers in each column opposite the ordinates represent the number of lines in the successive recalls. The total of each column is followed in order by the number of lines in the original selection, the per cent of the entire selection which was given in the first recall, the per cent added by subsequent recalls, the total per cent recalled, the number of attempts at recall and the number of days in which recalls were made.

Although the first recall was the best for each selection a number of subsequent recalls are not very inferior. The efficiency decreases with succeeding recalls but the decrease is by no means constant. On the average almost 50 per cent of the original selection was added after the first recall. It should be noted that at the end of practically every recall the subject felt certain that no more could be added then, although the feeling that more could be added by keeping at it long enough was reported a number of times. Indeed with enough trials, perfect reconstruction might not have been impossible. There seems to be no correlation between the first recall and the total of subsequent recalls. The individual with the second lowest per cent for first recall made by far the greatest gain in the total subsequent recalls. The ratios of the first recall to the total of subsequent recalls are .45, .75, 1.66, [illegible] .29. From the above facts one may conclude

AMOUNT, GAIN AND DISTRIBUTION OF RECALLS

	Wreck of the Hesperus —G. W.	Psalm of Life —G. M.	Psalm of Life —G. W.	Woodman, Spare that Tree—M. S.	Lady of the Lake —M. W.	Painter of Seville —C. M.
1	18	12	20	3	6	30.5
2	14	4	1	2	1	10.5
3	2	8	0	1	1.5	12.5
4	3	4	2	1	0	8
5	10	0	6	.5	.5	8
6	4		1	.5	.5	3
7	0		1	1		2
8	0		1	2		7
9	1.5		0	0		3
10	0			1		1
11	0			.5		4
12	3			.5		10
13	0			3		1.5
14	2					.5
15	0					5
16	0					2
17						2
18						4
19						5
20						12.5
Total number of lines given	57.5	28	32	16	9.5	132
Number of lines in poem	88	36	36	16	10	183
Per cent given in first recall	20.4	33.3	55.5	18.8	60	16.6
Per cent added in subsequent recalls	44.9	44.4	33.3	81.2	35	55.5
Total percentage given	65.3	77.7	88.8	100	95	72.1
Number of attempts at recall	16	5	9	13	6	20
Number of days covered in recalls	13	5	8	13	6	10

that the final recall is not a fair measure of retentiveness; much less is the first recall such a measure.

Because a number of the lines recalled from " Snowbound " by G. M. underwent so many changes and since he made no final reconstruction of all the recalls no attempt to enumerate his recall was made. Reference to his recalls readily show a comparatively high amount of the total recall given in the first recall. Those lines given in his first recall he had recalled very frequently during the five years interval but the succeeding lines had practically always failed to come up and had consequently been ignored. He reported the same for the first recall from the " Psalm of Life." G. W. whose first recall also was relatively high made a similar report. This corroborates experimental evidence of the value of recalling material once learned in assuring its retentiveness.

Recalling Names of Classmates

The writer G. C. M. tried to recall over a period of three months the names of his 76 classmates (1905). During that year he readily knew each one by name.

Feb. 24. He recalled the names of 31 and the faces of 3.

Feb. 26. He added 7 names and 3 faces.

Mar. 1. He added 6 correct and 2 incorrect names, the latter being marked *doubtful.*

Mar. 3. He began to recall by order of the alphabet and added 6 correct and 4 incorrect names (marked doubtful). Carter was given for Arter; Dixon for Hixon, and among the correct recalls was the name of one whose face only was recalled on Feb. 24th and 26th.

Mar. 6. He added 5 correct names, and 4 incorrect of which 3 were marked *doubtful.*

Mar. 8. He added 4 names. Arter was disguised in Alter but after a few minutes became Arter.

Mar. 10. He added one correct and one incorrect (*doubtful*) name. " Here I got the name of her whose face I had recalled several times before but for whom no other name but *Apples* and *Cider* would come up. She had often suggested these things to me while we were at school together. There was nothing in the sound of the name to suggest these words."

Mar. 12. He added two wrong words labelled *doubtful* and gave up in despair after 7 minutes although all previous recalls lasted for 15 minutes or more.

Apr. 22. He added 2 correct words, making a total of 62. Immediately after this recall he saw a picture which he had not seen during the experiment thus far, of all the class save one, and recognized only 56. " I later discovered that some I had recalled I could not recognize. I probably had recalled their names by mere sound associates. I did not look over the recalls in the meanwhile."

May 14. Sixty were recognized. G, S and B neither recalled nor recognized before, and one not recognized before were given. " The name of certain ones not recalled in look-over the picture, sometimes came up after I had passed on to others; for example, for the face of X, I could not get a name but I felt troubled because I was certain I really knew her name. After writing the names of the next three, not trying to think of X, her name flashed to my mind. This happened quite frequently in recognition. Of course the imperfection of the photograph might be responsible for some poor recognitions.

C. E. M. ran the same test on herself with practically the same results.

Some other studies of delayed recall may be of interest here. Colvin[1] concluded that "recall after 24 hours seems to be as good, on the whole, as immediate recall, when tested by the method of parts retained." Similar findings were reported by Radossawljewitsch.[2] Huguenin[3] found that the number of lines of poetry which can be recalled ten minutes after learning is less than the number reproducible at any subsequent time (up to eight weeks). The writer elsewhere found that of 47 boys, "21 gave more words in the second recall (after one hour) than in the first (immediate) recall,—with an average of two-tenths of a word more in the second than in the first recall." Forty-five out of 73 girls gave more in the second recall, with an average of .97 words more. For both sexes, from one to six correct words were added in the second or third recall.[4]

CONCLUSIONS

On the average the gain in the total subsequent recalls was about half as much as the original selections and about twice as much as the first recall.

Certainty for validity of recall was surprisingly safe, while certainty of inability to recall more at any time was highly unsafe. The more frequent the recalls, however, the more reliable the certainty for each. Recalls marked *doubtful* were incorrect as a rule but a few marked *wrong* were correct.

Although some errors persisted throughout most were corrected in subsequent recalls. More attempts at correction were made when all the recalls were reconstructed into a unit (final recall) and these attempts were nearly always successful. In all recalls the changes made were generally in the right direction. When alternatives were given the correct one almost invariably survived. Unreasonableness of a recall, at times challenged its correction. Errors were in words and rarely in content; the content, as a rule, was recalled before the words. In a few cases remnants from foreign selections obtruded themselves causing considerable confusion.

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[1] Colvin, S. S. Development of Imagination in School Children. *Psychol. Rev. Mon. Sup.*, 1909, 11, 85-124.

[2] Radossawljewitsch. Das Behalten und Vergessen bei Kindern und Erwachsenen nach experimentellen Untersuchungen. Leipzig, 1907.

[3] Huguenin, C. Revivescence paradoxale. *Arch. de psychol.*, 1914, 14, 379-383.

[4] Myers, G. C. Recall in Relation to Retention. *J. Ed. Psychol.*, 1914, 5, 119-130.

Feeling and general attitude of the subject toward the task factored highly in the recall-performance. Frequently the subjects reported that they could get more if they would keep at it but they could not drive themselves to try. A few were "captured" by the task so that it could not be dismissed at will. For them many remnants were recalled at the most unexpected moments.

Casual suggestions by sounds, sights, movements, ideas and feelings served to call up remnants in various proportions. For some subjects this was very pronounced while for others it was rare.

Recall of rhyming words proved a great aid in bringing up the rest of the respective lines.

The parts of the selections were not always recalled in order, nor were they reported in order, except in final reconstruction. Even within a stanza or a paragraph, recalls sometimes were made in the most perverted order.

There was practically no correlation between the first recall and the total subsequent recalls.

Selections with many recalls previous to the experiment were reproduced much better than those with few or no recalls.

These findings go a long way to substantiate the belief that, "We never wholly forget anything." Certainly we do not forget nearly so much as we think we do, and one's inability to recall facts on demand, is no proof that one has really forgotten them.

The recall time which has been almost wholly neglected in memory experiments is probably as important in measuring memory as the learning time. At best, persistency of the subject is a big factor in recall.

Pedagogically one may infer that if the teacher were skillful enough in questioning and in general presentation the most of what seems to have been forgotten by the pupil could be recalled as needed. Likewise if all that is studied could be properly correlated, the amount to be memorized would be comparatively small.

Motive, and deep seated interest on the part of the learner not only incite to greater activity, but, by evoking non-conscious recall they assure better results from the same expenditure of effort.

SOME SUGGESTIONS

In the light of this study a few suggestions are offered:

1. Wherever possible recall time should be measured or standardized.
2. Some method should be devised for recording quantity and quality of recalls during the various stages of the recall time.
3. Further correlations of recall time with learning time and with recall performance should be studied for each recall.
4. Incorrect recalls should be taken into consideration and their transition through subsequent recalls for each individual should be noted.
5. Whenever possible, a number of subsequent recalls should be made. The more numerous they are and the greater the time over which they are spread the more instructive they probably would be. Subsequent recalls should not be expected by the subject.
6. Some scheme might be devised whereby certain suggestions could be measured as to their influence in evoking correct recalls.
7. Perhaps delayed recall offers the richest field for memory study.

The writers acknowledge their indebtedness to Misses Martha Sullivan, Dorothy Tooker, Margaret Wharburton, and Grace White, students of Psychology of the Brooklyn Training School for Teachers, for the indispensable part they performed in this study.

AN EXPERIMENTAL STUDY OF ECONOMICAL LEARNING

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I

Introductory. The complaint that high school students are unable to perform fundamental computations with speed and accuracy is emphatic and of long standing. The time devoted to arithmetical instruction in the elementary school is disproportionate to the results obtained; and this will remain true while methods of teaching remain unsupported by methods of learning. What is true of the elementary school is only less true of the high school. It is not an exaggerated statement to affirm that the work in arithmetic could be accomplished in two-thirds of the time now required, if more economical methods of learning were employed and an ideal of speed and accuracy developed.

Meuman[1] emphasizes the necessity of supplementing methods of teaching by methods of learning: "This question (of economical learning) becomes the more pressing in modern times because our courses of study, in their attempt to comply with the increasing requirements of practical life, are becoming more and more exacting in the demands which they make upon the memory tasks of school children." Curriculum building is all in the direction of addition of subject matter without hint of compensation by subtraction. If over-burdening of the child is to be avoided it would seem that either subtraction must keep pace with this addition or the rate of learning must be increased by economy of method.

II

The Problem. The primary purpose of this study is to determine quantitatively the superiority of a method which eliminates unessential processes in the four fundamental operations. Otherwise stated, the purpose is to determine the loss in speed and accuracy accompanying the traditional methods of teaching the four fundamental operations. The two methods will be referred to in this study as the 'traditional' and 'economical' methods.

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sychology of Learning, p. xiv.

245
987
621
876
912
543
———
4,184

The solution of the problem in the margin may serve as an illustration of the waste and fatigue in the prevailing method. When each digit and connecting terms are definitely expressed no less than seventy-one articulations are required. This number is increased by sixteen when the carrying process is formally expressed. Various abbreviations of this procedure are employed but it is exceptional to find a first year high school pupil who, by merely naming results, employs as few as seventeen in the solution.

The difference between the eighty-seven and seventeen expressions represents so much waste and fatigue. Other things being equal, the speed attained should increase with an approach to the smaller number of words. Special attention to the training of the pupils in group addition would enable them further to reduce this number to a minimum of ten words or less. In this study pupils were not trained in group additions for the reason that it would obscure the present problem and for the additional reason that the manner of grouping would not be uniform for all pupils. The traditional and the economical methods would both be proportionally shortened by application of group addition.

Experiments by Pintner,[2] Huey,[3] and Dearborn,[4] show conclusively that articulation in silent reading hinders both rapidity and efficiency of thought getting. As in reading, so in arithmetic, the direct linking of visual forms of digits and groups of digits, without the cumbersome and fatiguing audito-motorizing mechanism, leads to greater rapidity and accuracy in arithmetical computation.

III

Procedure. The Courtis Tests in Arithmetic were given to seventy-six first year high school pupils. From this number sixty-four were selected and divided into two equal groups, each group having approximately the same average initial speed and accuracy. Group T was drilled in the traditional method and Group E was trained in the economical method. Both groups were given identical problems in each of the four fundamental operations. All objective conditions were equalized as completely as possible.

Each group was urged to solve the entire list of twenty-eight problems as rapidly and accurately as possible. The instruction given Group E differed from that given Group T only in the added direction that they must 'think results only.' The first column in the illustrative problem given above, therefore, would be read 5, 11, 12, 19, 24, and the second column 6, 7, 14, 16, 24, 28, 8. It should be stated that this group was frequently reminded to "think in terms of results only," since there was evidence that old habits functioned persistently.

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[2] 'Silent Speech,' *Pintner, Psychological Review,* 1913.

[3] 'The Psychology and Pedagogy of Reading,' *Huey,* 1900.

[4] 'The Psychology of Reading,' *Dearborn, Archives of* Vol. 4, 1906.

There were eight work periods for each group. To equalize the amount of practice for the two groups, and for all the individuals, the work-limit[5] method was chosen in preference to the time-limit method. During a given work period each pupil solved seven problems in each of the four fundamental operations. The two groups practiced at different periods; Group T at the beginning, Group E at the close of the session, thus giving the former group what is commonly regarded as the more favorable hour.

IV

Experimental Material. It was especially desirable to make all the problems of the various sets of equal difficulty. To this end, the nine digits were made to recur with the same frequency. It is probable that such usage of the digits secures a desirable distribution of fatigue to the problems of a given set and also makes possible a uniform basis of comparison of work accomplished in the successive work periods.

Each problem in addition consisted of six three-digit numbers. The problems in subtraction involved two nine-digit numbers. In multiplication, the multiplier was a one-digit number in order to avoid the increase of addition processes by adding several partial products. In division, the divisor was a single digit so as to confine the processes as narrowly as possible to division only. The digits one and two were not used as multipliers or divisors.

V

Results. To provide a standard measurement of results, the Courtis Tests were given again after the last work period.

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[5] To enable pupils to record the time required for the completion of the addition, subtraction, multiplication and division lists respectively, the time in seconds was recorded by the instructor every ten seconds in large figures on the blackboard. As soon as each set of problems was finished the last recorded time was written by the pupil in a place designated at the end of the set. This plan consumed none of the pupil's time in computing the number of seconds required for the several sets and removed a sou[illegible] time required for each of the four sets of [illegible] the experimenter from the data sh[illegible]

The relative efficiency of the two groups at the beginning of the experiment is shown in Tables I and II.

TABLE I

AVERAGES FOR COURTIS TESTS IN THE FOUR FUNDAMENTAL OPERATIONS

Group	INITIAL TEST				FINAL TEST				Improvements in per cent	
	Attempts		Rights		Attempts		Rights			
	Med.	Var.	Med.	Var.	Med.	Var.	Med.	Var.	Att.	Rts.
T....................	12.9	.24	12.0	.24	14.0	.20	11.7	.32	8.5	-2.5
E....................	12.5	.25	11.0	.28	16.8	.19	14.4	.24	34.4	30.9
Difference............	.4	.01	1.0	.04	2.8	.01	2.7	.08	25.9	33.4
Superiority of E in (%)	-3.1	-4.2	- 8.3	-16.7	20.0	5.00	24.8	25.00	23.1	33.1

The former shows averages for initial and final Courtis tests with a comparison of efficiency and improvement of the two groups. The median number of problems attempted by Group T in the initial Courtis test was .4 higher than those by Group E. The median number of problems correctly solved by Group T was also higher by 1. The variability of Group T was less than that of Group E. This gives Group T an initial superiority over Group E. Table II gives a comparison of the average time required by the two groups to complete the seven problems in each of the four fundamental operations during the eight work periods. This table does not indicate an initial advantage for Group T, the average total time of Group E being five seconds less than that of Group T.

TABLE II

COMPARISON OF GROUP AVERAGES IN SECONDS
(With penalty added for errors)

Work Periods	GROUP T					GROUP E				
	+	−	×	÷	Total	+	−	×	÷	Total
1	172	199	211	218	800	169	213	200	213	795
2	141	192	193	172	698	169	193	195	177	734
3	162	148	224	219	753	183	168	228	246	825
4	138	134	186	191	649	161	152	205	190	708
5	161	196	188	177	722	164	175	172	172	683
6	154	182	199	192	727	144	141	177	173	635
7	140	187	185	181	693	129	173	173	179	654
8	144	186	183	172	685	125	163	163	148	599

The ordinates of the curves in Fig. 1 represent group averages in seconds for the eight work periods in the four fundamental operations. Each point of the curve, therefore, indicates the average number of seconds required by the group to solve the seven problems.

It was impossible, of course, to equate perfectly the intelligence of the two groups. In order to test more effectively the economic method, the values for the first work period of the various curves shown in Figs. 1, 2 and 3 were derived by giving both groups practice in the method which applied to Group T only in subsequent work periods. Obviously, this gives Group T a handicap of one work period in addition to the practice afforded by the initial Courtis test. A glance at the curves b and c, Fig. 4, indicates that the above mentioned handicap was unnecessary, since the curve for Group T already shows a superiority over Group E, excepting at point Att. in multiplication and at points, Att. and Rt. in division.

The handicap operates to the disadvantage of Group E in two ways,—it facilitates the efficiency of Group T on the one hand and accentuates the disadvantageous habits in Group E on the other.

The data in Tables III and III (a) for the initial Courtis Tests show that the median number of problems attempted by Group T was 1.2 higher than Group E in addition, .7 higher in subtraction, .3 lower in multiplication and .2 lower in division, giving Group T a total initial advantage of 1.4 "attempts." The median number of problems correctly solved by Group T in the initial Courtis Test was 1.6 higher than in Group E in addition, .7 in subtraction and 2.3 in multiplication and lower by .6 in division, giving Group T a total initial advantage of 4 "rights." These tables also show the initial variability in the number of problems attempted by Group E to be greater by 5% in addition, 5% in subtraction, equal variability in multiplication and 5% less in division,—an average initial disadvantage of 1.25% for the four fundamental operations. The variability of problems correctly solved by Group E was initially greater by 8% in addition, 9% in subtraction, 10% in multiplication and less by 9% in division,—giving Group T an average initial advantage of 4.5%.

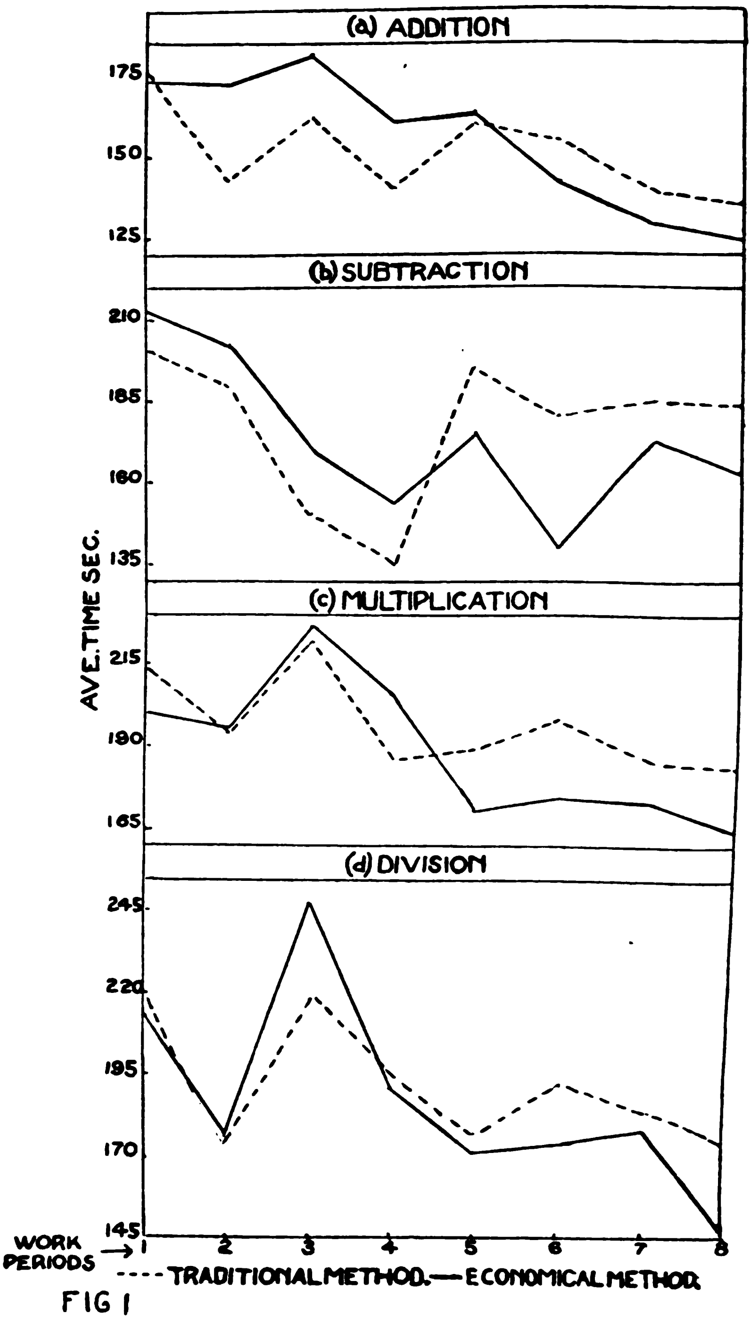
(a) ADDITION
175
150
125
(b) SUBTRACTION
210
185
160
135
(c) MULTIPLICATION
215
190
165
(d) DIVISION
245
220
195
170
145
AV.E. TIME SEC.
WORK PERIODS →
1
2
3
4
5
6
7
8
---- TRADITIONAL METHOD. — ECONOMICAL METHOD.

FIG 1

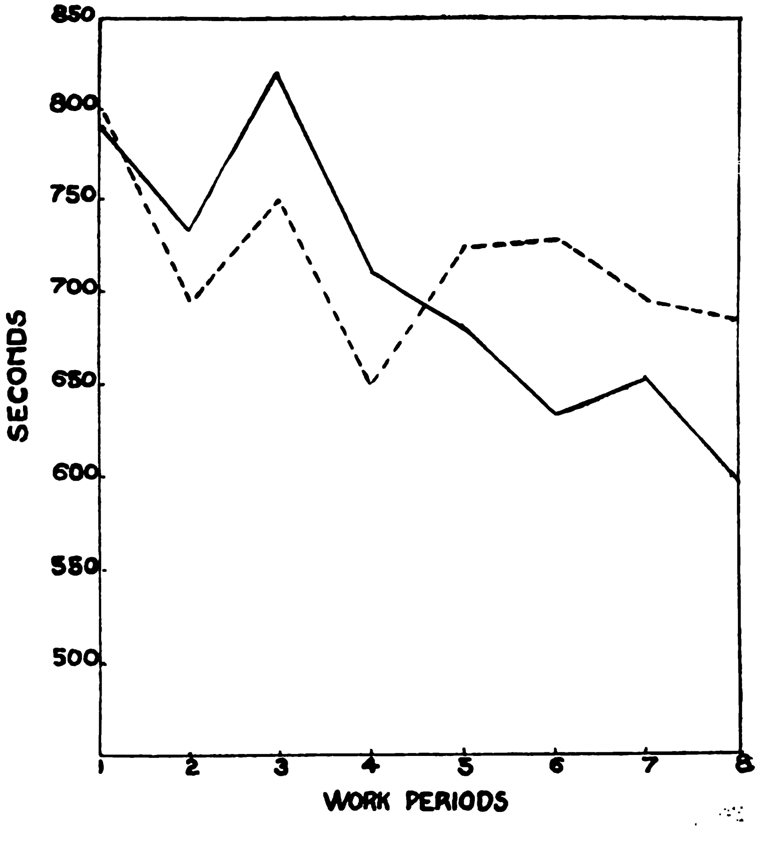

FIG. 2 LEARNING CURVE, showing total time each work period
GROUP E = ——— GROUP T = - - - - - -

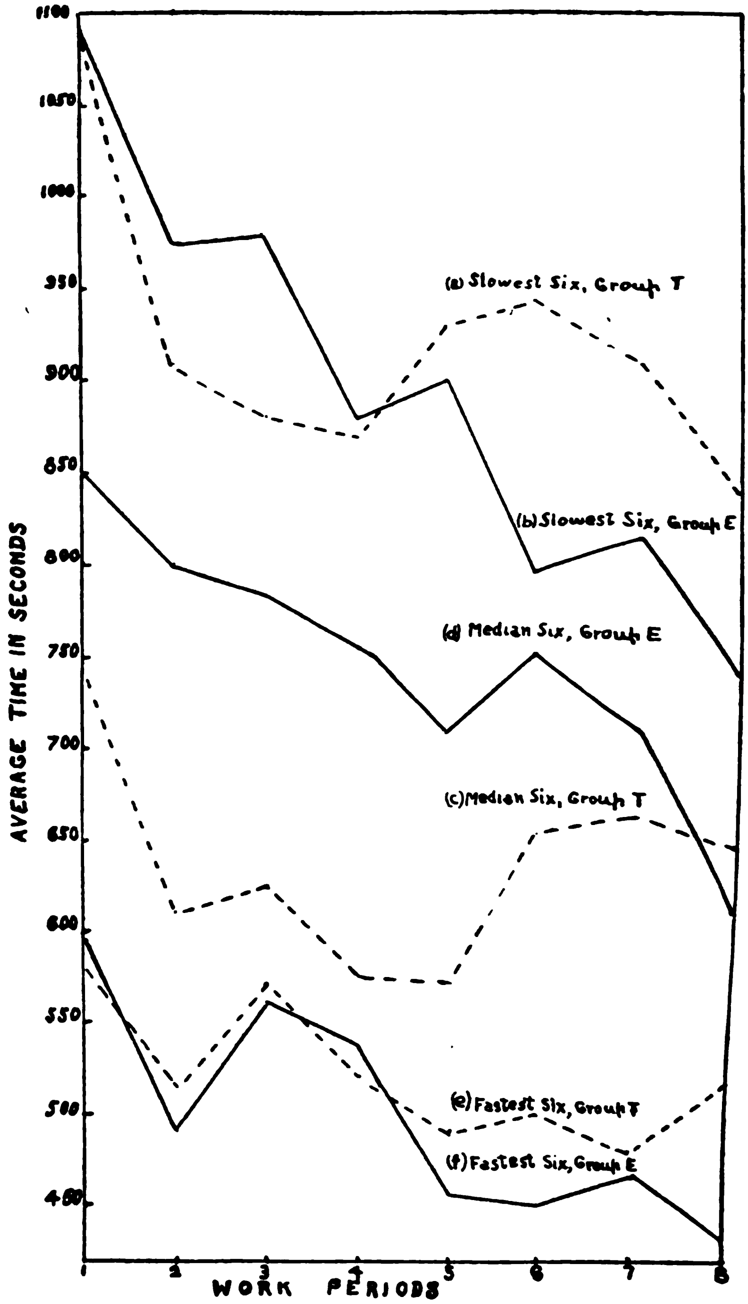

FIG. 3. LEARNING CURVES
FOUR FUNDAMENTAL OPERATIONS COMBINED

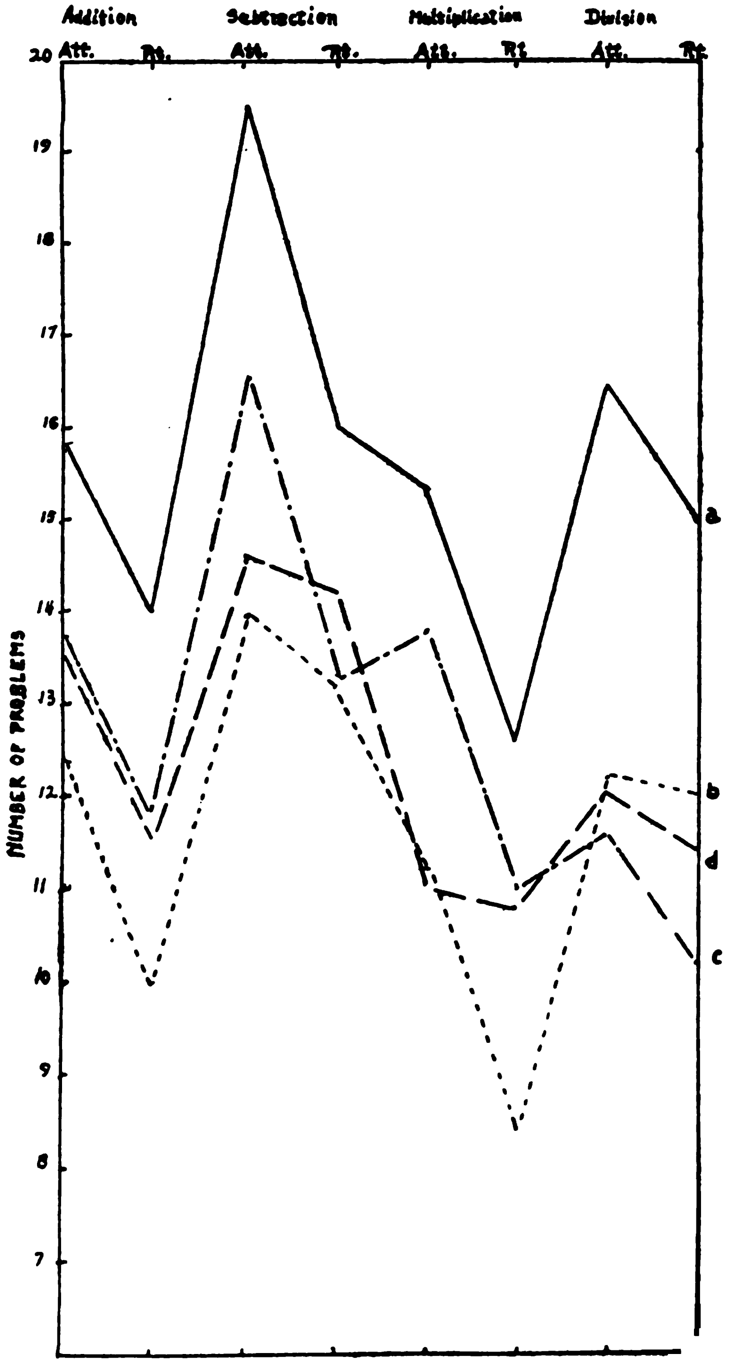

FIG. 4 INITIAL AND FINAL COURTIS TES

Att., Number of Problems Attempted. Rt. Number of Pr

—— Group E - - - - - b, Initial Test

- oup T. —— —d. Initial T

TABLE III

INITIAL COURTIS TEST

Group T

	ADDITION					SUBTRACTION					MULTIPLICATION					DIVISION				
	Atts.			Rts.		Atts.			Rts.		Atts.			Rts.		Atts.			Rts.	
	Frequency	Deviation	Score	Frequency	Deviation	Frequency	Deviation	Score	Frequency	Deviation	Frequency	Deviation	Score	Frequency	Deviation	Frequency	Deviation	Score	Frequency	Deviation
			24					24					24					24		
	2		23			1		23					23					23		
			22					22					22			1		22	1	
			21			1		21	1				21					21		
	2		20	3		1		20	1				20					20		
	1		19					19	1				19			1		19	1	
			18			2		18			1		18			2		18	2	
	1		17			4		17	1		1		17			1		17	1	
	3		16	2		4	7	16	7	10			16	1		1		16	1	
	1	5	15	2			4	15	4	9	3		15	2		2	3	15		
	3	6	14	2	5	6	6	14	2	2	3		14			1	3	14	2	4
	6	6	13	3	4	4		13	5		5	7	13	1		3	8	13	1	2
	3		12	1	6	3		12	3		3	5	12	4	6	4	4	12	4	7
	4		11	6	6	1		11	2		7	7	11	7	10	5		11	4	4
	2		10	5		2		10	2		2		10	5	5	2		10	3	
	2		9	1				9	1		2		9	3		1		9	1	
	1		8	3		3		8	1		2		8	2		3		8	2	
	1		7	1				7	1		2		7	7		2		7	3	
			6	1				6			2		6			3		6	2	
			5					5					5	1		1		5	4	
			4					4					4					4		
			3	1				3					3					3		
			2	1				2					2					2		
			1					1					1					1		
			0					0					0					0		
Ap. M..	13			11		14			14		11			10		12			11	
Cor.....	.7			.6		.7			0		0			.8		0			.5	
Med....	13.7			11.6		14.7			14		11			10.8		12			11.5	
M. D...	3			2.2		3			2.6		2.7			2.3		3.6			4	
Var.....	.22			.19		.20			.19		.24			.21		.30			.35	

Ap. M. = apparent median; Cor. = correction; Med. = median; M. D. = median deviation; Var. = Variability

TABLE III (a)

INITIAL COURTIS TEST

Group E

	ADDITION					SUBTRACTION					MULTIPLICATION					DIVISION				
	Atts.			Rts.		Atts.			Rts.		Atts.			Rts.		Atts.			Rts.	
	Frequency	Deviation	Score	Frequency	Deviation	Frequency	Deviation	Score	Frequency	Deviation	Frequency	Deviation	Score	Frequency	Deviation	Frequency	Deviation	Score	Frequency	Deviation
			24					24					24					24		
			23					23					23					23		
			22					22					22					22		
	1		21					21					21					21		
	2		20			4		20	2				20					20		
			19			2		19					19					19		
	1		18	2		3		18	1				18			1		18		
			17	1			2	17	4		2		17					17		
	1		16			4	9	16	1	3	2		16			1		16	1	
	4	8	15			2	6	15	1	4	1		15			5	7	15	4	6
	3	5	14			1	1	14	3	8	1		14	2		2	5	14	3	5
		2	13	2		4		13	3	3	4	10	13	2		3	6	13	2	5
	7	7	12	4	9	5		12	5		3	7	12	2		5	5	12	6	6
	2		11	3	7	2		11	3		3	3	11	2		3		11	3	
	2		10	3	3	4		10	2		4		10	2	7	3		10	2	
	4		9	4		1		9	4		6		9	5	9	2		9	2	
	2		8	5				8			2		8	2	2	4		8	4	
	2		7	1				7			4		7	4				7	2	
	1		6	3				6	2				6	5		2		6		
			5					5					5	4				5		
			4	1				4					4	1		1		4	1	
			3	3				3					3					3	2	
			2					2					2	1				2		
			1					1					1					1		
			0					0					0					0		
Ap. M..	12			10		14			13		11			8		12			12	
Cor.....	.5			0		0			.3		.3			.5		.2			.1	
Med....	12.5			10		14			13.3		11.3			8.5		12.2			12.1	
M. D...	3.4			2.7		3.5			3.7		2.7			2.6		3.1			3.2	
Var.....	.27			.27		.25			.28		.24			.31		.25			.26	

TABLE IV

FINAL COURTIS TEST

Group T

	ADDITION					SUBTRACTION					MULTIPLICATION					DIVISION				
	Atts.			Rts.		Atts.			Rts.		Atts.			Rts.		Atts.			Rts.	
	Frequency	Deviation	Score	Frequency	Deviation	Frequency	Deviation	Score	Frequency	Deviation	Frequency	Deviation	Score	Frequency	Deviation	Frequency	Deviation	Score	Frequency	Deviation
	1		24			1		24					24					24		
	1		23	2		1		23					23					23		
	1		22			1		22	2				22					22		
			21			2		21			1		21					21		
			20					20	1				20			1		20		
			19	1		1		19					19					19	1	
			18			2	7	18	2				18			1		18		
	2		17			5	6	17	1		5		17			1		17	1	
	3		16	1	4	5	5	16	3	10	2	6	16	1				16	1	
	2	5	15	1	1	1		15	3	5	1	4	15	1		1		15	1	
	4	6	14	4	6	5		14	1	2	5	6	14	4		3		14	2	
	6	6	13	1	3	1		13	3	3	5	5	13	3	6	3	7	13	3	6
	2		12	5	5	3		12	1		1		12	2	7	4	8	12	2	6
	3		11	2				11	2		3		11	4	4	3	3	11	1	4
	2		10	2		2		10	7		4		10	5		4		10	4	4
	4		9			1		9	1		1		9	3		4		9	3	
	1		8	3		1		8	2		1		8	1		3		8	4	
			7	6				7			1		7	2		3		7	3	
			6	1				6	1		2		6			1		6	2	
			5	3				5					5	2				5	1	
			4					4	1				4	3				4	2	
			3					3	1				3					3	1	
			2					2					2	1				2		
			1					1					1					1		
			0					0					0					0		
Ap. M..	13			12		16			13		13			11		11			10	
Cor.....	.8			0		.6			.3		.8			0		.6			.3	
Med....	13.8			12		16.6			13.3		13.8			11		11.6			10.3	
M. D...	2			4.5		2.9			3.7		3.3			3		2.6			3.5	
Var.....	.15			.37		.18			.28		.24			.27		.22			.34	

TABLE IV (a)

FINAL COURTIS TEST

Group E

	ADDITION					SUBTRACTION					MULTIPLICATION					DIVISION				
	Atts.			Rts.		Atts.			Rts.		Atts.			Rts.		Atts.			Rts.	
	Frequency	Deviation	Score	Frequency	Deviation	Frequency	Deviation	Score	Frequency	Deviation	Frequency	Deviation	Score	Frequency	Deviation	Frequency	Deviation	Score	Frequency	Deviation
			24			4		24	1				24			1		24		
	1		23			3	6	23	1				23					23	1	
	1		22			3	6	22	3		1		22					22		
			21	1			3	21	2		1		21	1		1		21		
			20	1			1	20	2	3	1		20	1		1		20		
	3		19			5	5	19	1	3	3		19			2		19	2	
	4	7	18			1		18	1	4	1		18	1		4	6	18	5	6
	2	6	17	3	9	3		17	2	6	1	5	17	1		1	3	17	1	3
	3	6	16	3	6	3		16	2	2	4	9	16	1		8	8	16	3	7
	4	4	15	2	6	3		15	4		5	5	15		6	2		15	3	3
	3		14	3	3	3		14	3		5		14	2	4	2		14	4	
	4		13	4		1		13	2		4		13	7	9	2		13	2	
	3		12	3		1		12	1		3		12	3	3	2		12	1	
	4		11	6				11	2		3		11	2		1		11	4	
			10	5				10	2				10	2		2		10	1	
			9	1				9	2				9	6		1		9	2	
			8					8	1				8	3		1		8	1	
			7					7					7	2		1		7	2	
			6					6					6					6		
			5					5					5					5		
			4					4					4					4		
			3					3					3					3		
			2					2					2					2		
			1					1					1					1		
			0					0					0					0		
Ap. M..	15			14		19			16		15			12		16			15	
Cor.....	.8			0		.4			0		.4			.6		.4			0	
Med....	15.8			14		19.4			16		15.4			12.6		16.4			15	
M. D...	3.1			3.2		4.3			4.6		2.6			3.1		3			3.7	
Var.....	.20			.23		.22			.27		.17			.24		.18			.25	

In the final Courtis Tests we find, Tables IV and IV (a) that the median number of problems attempted by Group E exceeds that of Group T by 2 in addition, 2.8 in subtraction, 1.6 in multiplication and 4.8 in division,—a superiority of 11.2 problems. The median number of problems correctly solved by Group E exceeds that of Group T by 2 problems in addition, 2.7 in subtraction, 1.6 in multiplication and 4.7 in division,—a total superiority of 11 problems. In the final test the variability in the number of " attempts " was greater for Group E by 5% in addition, 4% in subtraction and less by 7% in multiplication and by 4% in division,—an average superiority of 5% for Group E. The variability of the number of " rights " for Group E in the final Courtis Test was less by 14% in addition, 1% in subtraction, 3% in multiplication, and 9% in division,—an average of 6.7% less variability for Group E.

With respect to the number of problems attempted at the close of the eight practice periods, Groups T and E improved 8.5% and 34.4% respectively; with regard to the number of problems correctly solved, Group T lost 2.5% while Group E gained 30.9%. (For summary see Table I.)

Fig. 9 graphically sets forth the relation of the medians and variability as given in Table I. The medians of the initial attempts for all four fundamental operations show very slight differences between Groups T and E. The arrows practically fall within a vertical line. This is not true for the medians of the final attempts and rights. Here the medians for Group E in both attempts and rights exceed that of Group T by 2.8 abscissae points. The median of final " rights " for Group T occupies a position .3 abscissae points farther to the left than its position in the initial test, showing a decrease in accuracy with the increase in speed.

The marked superiority of the economical method is clearly seen in curve a, Fig. 4. The greatest superiority is in division and the least in addition.

Figs. 5-9 show in a striking manner the decrease in variability under the economical method, the graphs becoming more compact around the median. This characteristic is most marked in the addition graphs (Fig. 5), showing decreased variability both in the number of final attempts and in the number of rights for Group E as previously stated. Group E excels Group T at and after the sixth work period (Table II) in addition, at the fifth period and thereafter in subtraction and multiplication, and at the fourth period and thereafter in division. The irregular rates of improvement for both groups, apparent in the columns of totals (Table II) and in the graphs (Fig. 1), derived from Table II, indicate uncontrollable changes in attitude of pupils probably due to flagging interest and to unfavorable bodily and environmental conditions.

Individual records as well as fluctuations of group averages in the case of Group E indicate that the arithmetical habits acquired in the elementary school, asserted themselves more insistently in the 3rd and 7th work periods. Here we find a marked increase in the average time required for completion of the problems when a decrease should have been expected. Statements of some of the members of the group corroborate the conclusion.

The greatest amount of improvement is found in division of Group E (Table II). Here the average time required to solve the seven problems of the eighth work period was 65 seconds less than the time required in the first work period (213-148). The corresponding improvement for multiplication, subtraction and addition in Group E was 37, 50 and 44 seconds respectively. Improvements for Group T in the same order as given above for Group E were 46, 28, 13 and 28 seconds. The time required to complete the third work period of Group E was 246 seconds. This time exceeds that of any other work period of either group for any of the four fundamental operations. Here the average time was 27 seconds more than in the corresponding period and operation for Group T. This time, (246 seconds), for Group E was 33 seconds more than in the first work period, 69 seconds more than in the second work period and 98 seconds more than in the last work period. These figures are significant in view of the fact that it was at this point that the pupils complained most of the interference of their former habits. Moreover it was in the third period that the economical method was first really used by Group E. The pupils confessed to having used the traditional method during the second period because they found their desire for speed led them, unconsciously, to break away from the economical method and to lapse into the traditional method.

The greater difficulty in learning division and the greater final improvement agrees with results of Chapman.[6] He found that improvement with practice is always greatest in the more complicated processes. Division involves a greater variety of processes than any of the other three fundamental operations.

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[6] Chapman, James Crosby, Thesis (Ph.D.), Columbia, 1914.

The irregular character of the learning curves for Group T is probably due to the waxing and waning of interest incident to the continuance of a method devoid of novelty. An effort was made to iron out the irregularity by giving each pupil a copy of his group record for the previous work periods, by calling attention to the achievements of superior pupils and by urging a maximum of effort on the part of all. The same incentives were given to both groups.

The curves in Fig. 2 are composites of curves a, b, c, and d of Fig. 1. Each point in the curves shows the total time required in each work period to complete the problems in addition, subtraction, multiplication and division. The efficiency of Group E, as shown by these curves, unmistakably exceeds that of Group T after the fourth work period. Accuracy is incorporated in the graphs of Figs. 1, 2 and 3 by adding to the group averages the number of seconds which would have been required, at the average speed, to correct all errors. Group T had 9/10 per cent of error and Group E 7/10 per cent for the average of the eight work periods. The total number of errors made by each group during each of the work periods and the average number of errors per pupil are given in Table V and the former is graphically shown in Fig. 10.

TABLE V

TOTAL ERRORS AND AVERAGE ERRORS PER PUPIL

Groups	WORK PERIODS															
	1		2		3		4		5		6		7		8	
	T.	Av.	T.	Av.	T.	Av.	T.	Av.	T.	Av.	T.	Av.	T.	Av.	T.	Av.
T.	180	5.6	157	4.9	180	5.6	140	4.4	147	4.6	183	5.7	187	5.9	191	6.0
E.	173	5.4	135	4.2	100	3.1	112	3.5	134	4.2	119	3.4	152	4.7	140	4.4

The curves in Fig. 3 show the efficiency of the six initially fastest, six median and six slowest pupils of both groups. The increase of speed with practice is most conspicuous in curve b, the learning curve for the slowest six pupils of Group E. The difference between the average time in the initial and final work periods is 335 seconds (1086-751 sec.). The median six of this group decreased the average time 235 seconds (846-611), while the fastest six decreased it 165 seconds (595-430). The corresponding decreases in Group

T were 246, 103 and 55 seconds. These differences represent the time saved in the eighth work period as compared with the first.

The percentage of increase in speed was nearly uniform throughout Group E, while only the slowest pupils in Group T made noteworthy improvement with practice; but this improvement was much less than that made by the corresponding six pupils of Group E. The six slowest, the six median and the six fastest pupils of Group E surpassed the corresponding pupils of Group T at the fifth, eighth and fifth work periods respectively. Expressed in per cent, the slowest, median and fastest pupils of Group T improved 23, 13 and 9 per cent respectively while those in Group E improved 31, 28 and 28 per cent.

TABLE VI

Groups of Six Pupils	Improvement in Seconds		Improvement in Per Cent	
	Group T	Group E	Group T	Group E
Slowest	246	335	23	31
Median	103	235	13	28
Fastest	55	165	9	28

SUMMARY AND CONCLUSIONS

(1) According to the final Courtis Tests the group of pupils working economically shows the following striking superiority over the group working traditionally: (a) in the number of problems *attempted*, 11.2; (b) in the number of problems correctly solved, 1; (c) 6.7% less in variability; (d) with respect to the number of problems attempted at the close of the eighth work period 33.4%. (Group T actually lost 2.5%.)

These statistics gain enormously in importance when it is remembered that (1) the pupils working traditionally showed a decided superiority in the initial Courtis Tests, and (2) that Group E employed the method of Group T for the first of the eight work periods.

(2) Of the four fundamental operations, division shows the greatest absolute improvement in Group T as well as Group E. These values for Groups E and T are 65 and 46 seconds respectively. These results confirm the observations made by Chapman referred to above.

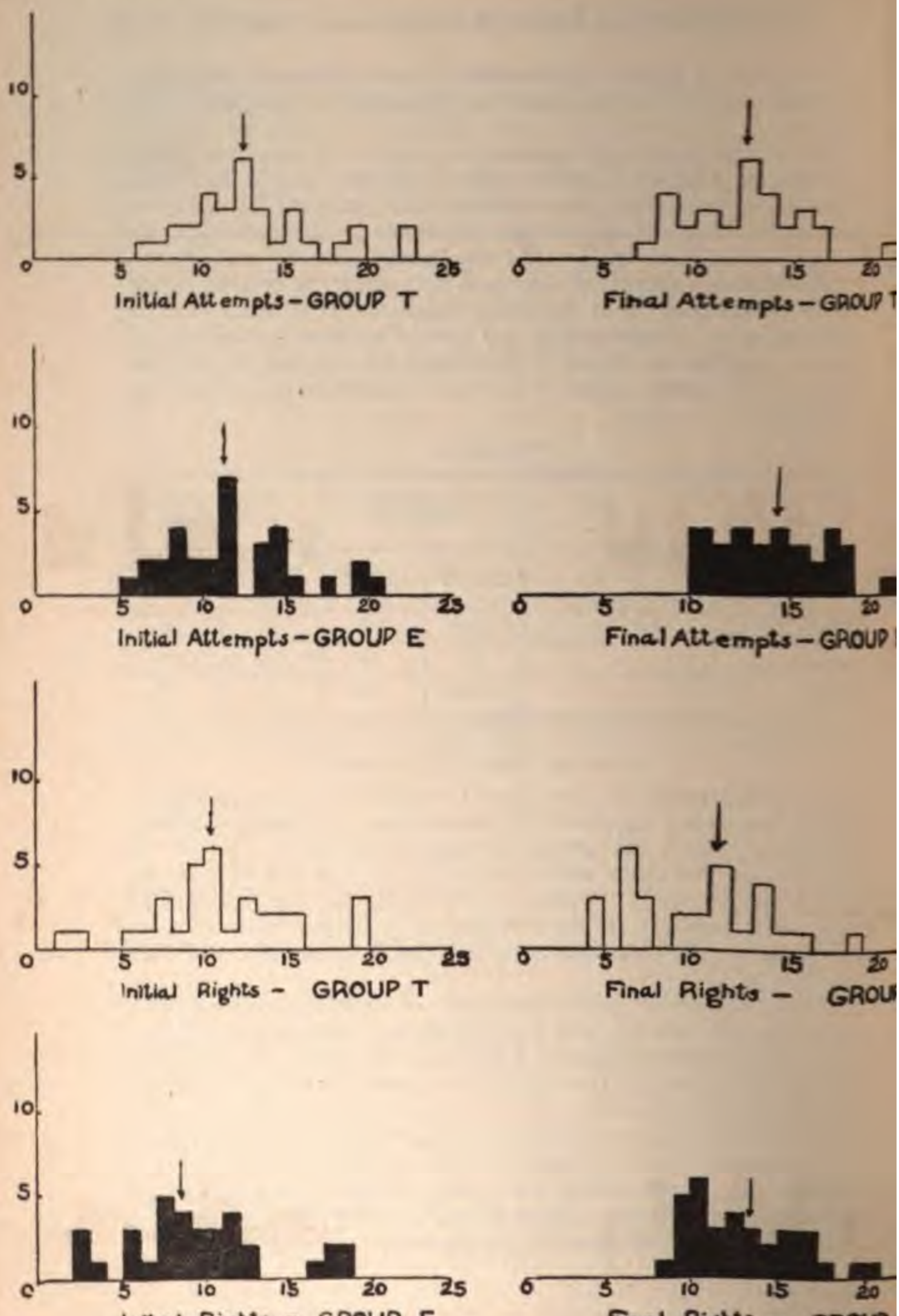
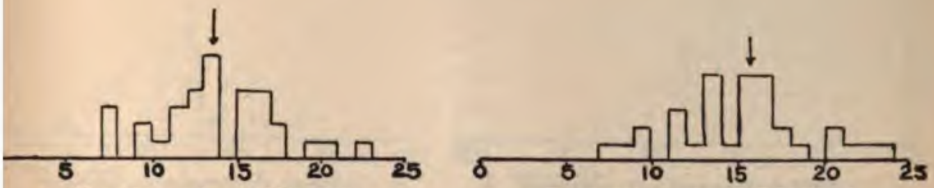
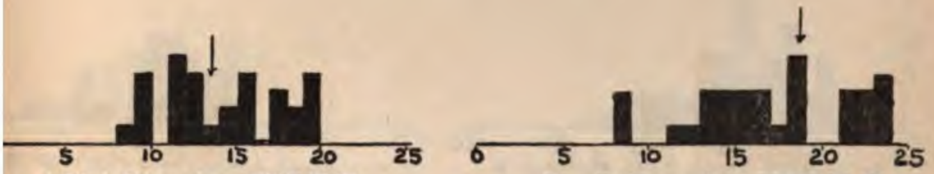


FIG 5, COURTIS TESTS IN ADDITION

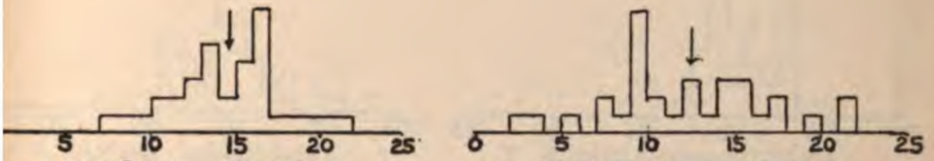
ABSCISSAS = PROBLEMS ORDINATES = PUPILS



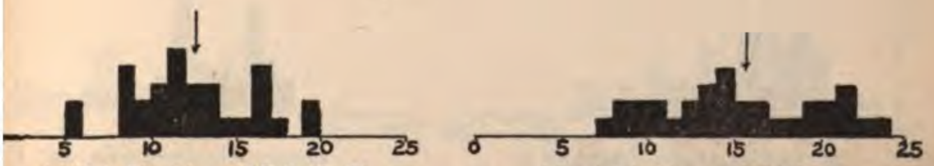
Initial Attempts - GROUP T Final Attempts - GROUP T



Initial Attempts - GROUP E Final Attempts - GROUP T



Initial Rights - GROUP T Final Rights - GROUP T



Initial Rights - GROUP E Final Rights - GROUP E

FIG. 6, COURTIS TESTS IN SUBTRACTION

ABSCISSAS = PROBLEMS ORDINATES = PUPILS

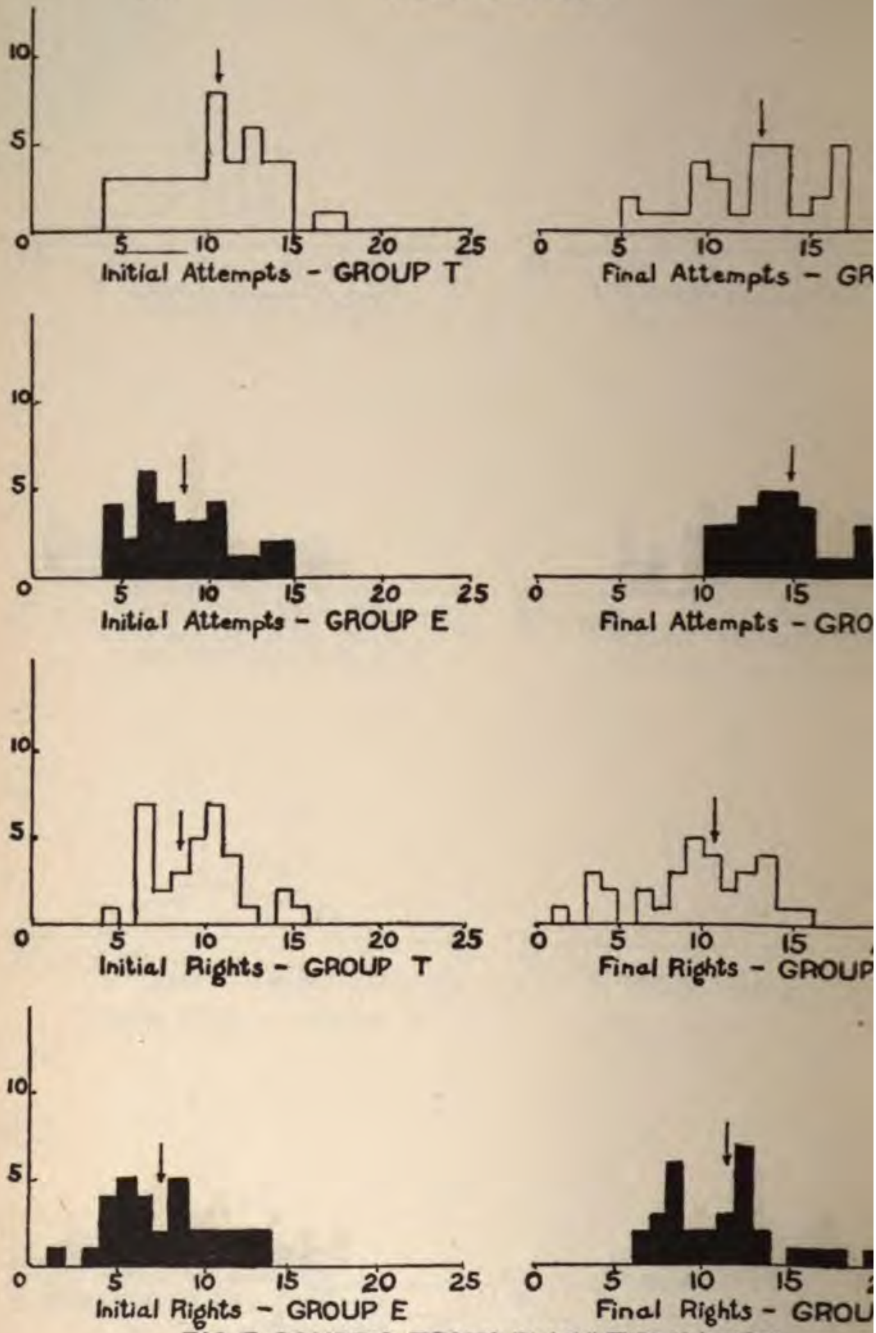
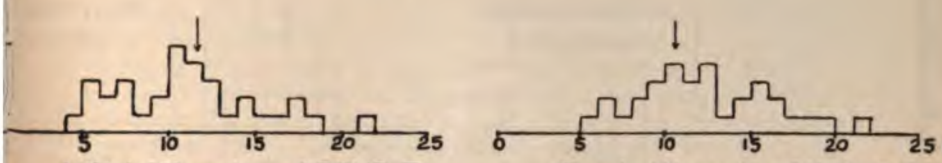
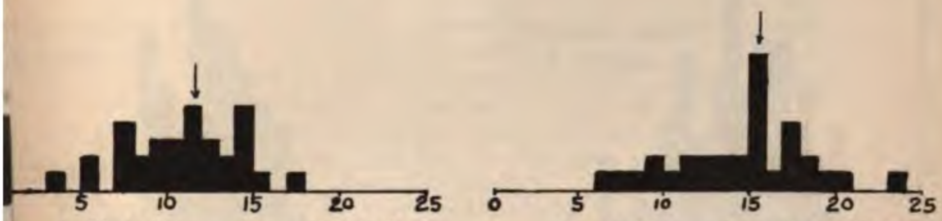


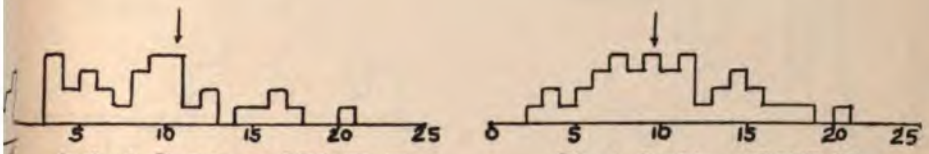
FIG. 7, COURTIS TESTS IN MULTIPLICATION
ABSCISSAS = PROBLEMS ORDINATES = PUPILS



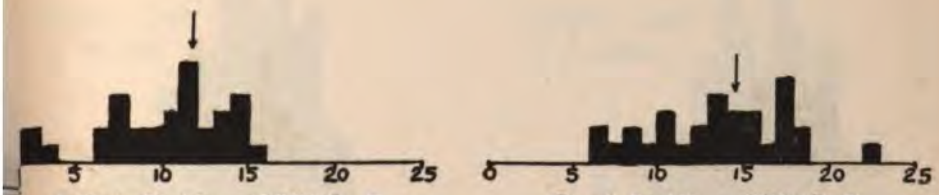
Initial Attempts - GROUP T   Final Attempts - GROUP T



Initial Attempts - GROUP E   Final Attempts - GROUP E



Initial Rights - GROUP T   Final Rights - GROUP T



Initial Rights - GROUP E   Final Rights - GROUP E

FIG. 8, COURTIS TESTS IN DIVISION
ABSCISSAS = PROBLEMS   ORDINATES = PUPILS

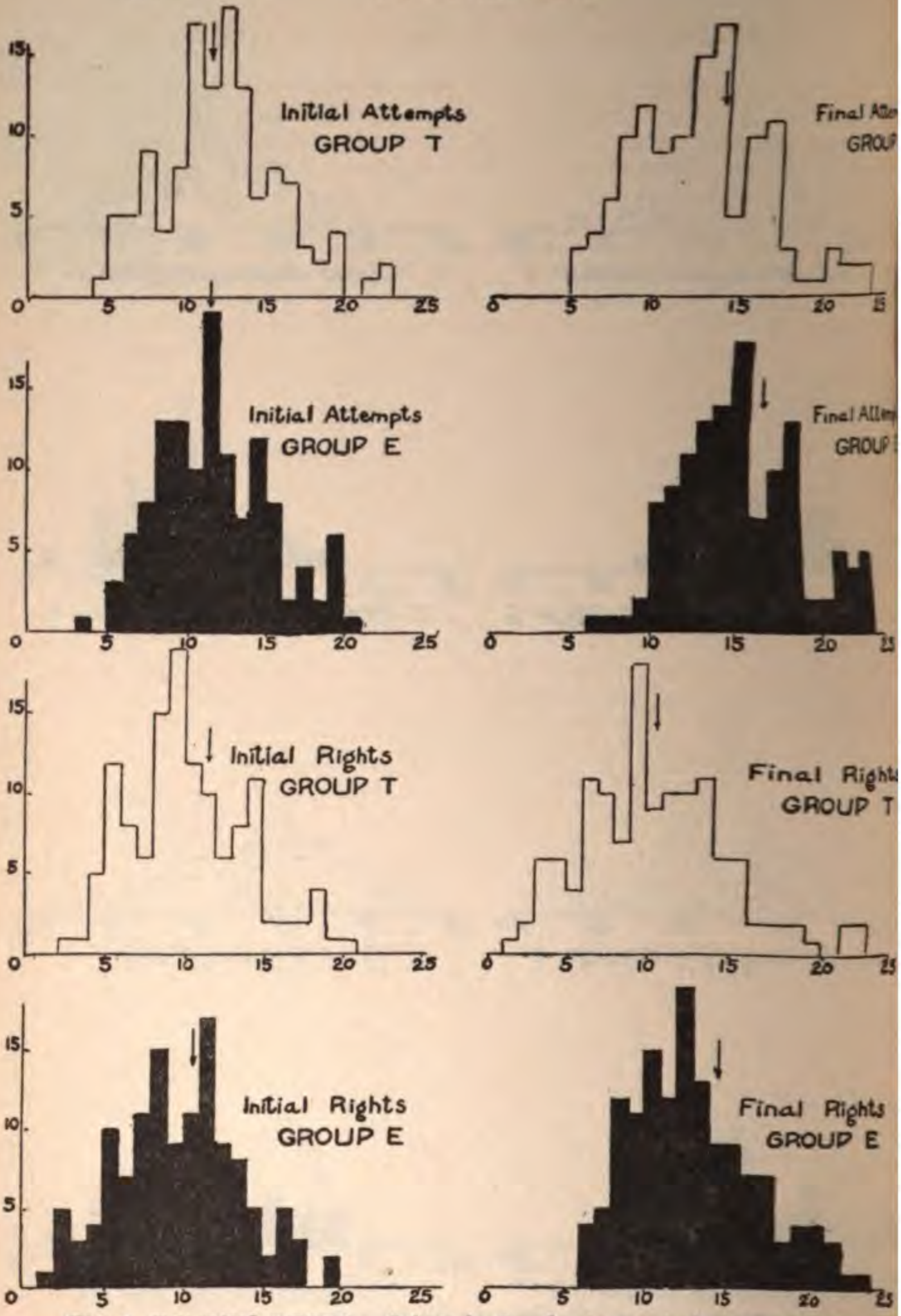


FIG. 9, COURTIS TESTS — Totals for four fundamental operations

(3) Individual records and group averages of Group E indicate that the interference of established habits of work manifest themselves irregularly. The phenomenon of interference was greatest in the third and seventh work periods.

(4) The superiority of the economical group is again shown in the reduction of the amount of time required to solve the problems of the last work period as compared with the first period. In this respect the economical group shows a higher efficiency than Group T by 19, 9, 37 and 16 seconds for division, multiplication, subtraction and addition respectively.

(5) The gain in speed of Group E over the corresponding pupils of Group T in the four fundamental operations combined, of the six slowest, six medians and six fastest pupils, is 90, 132 and 110 seconds respectively.

The adoption by elementary and secondary schools of the method of arithmetical computations employed by Group E would eliminate an important factor of waste and make possible a considerable reduction in the time now devoted to arithmetic. In the four fundamental operations it appears that pupils should be permitted to think in terms of results only and restrict, so far as possible, the audito-motorizing mechanism.

The economical method should be employed as soon as possible to prevent the formation of 'interference' habits and for the reason that the economical method is more favorable to the development of habits of attention.

NEW LABORATORY EQUIPMENT

By CHRISTIAN A. RUCKMICH,
University of Illinois

For many years the American Psychological Association has arranged for the display of apparatus at its annual meetings. Scattered articles on apparatus have appeared also from time to time in our psychological journals. Both of these methods of exposition reveal in its material aspects the growth of empirical psychology, but the author is strongly of the belief that there is not yet enough mutual coöperation in the way of improving our empirical resources in general and our laboratory equipment in particular. Committees have been at work on our laboratory methods, our laboratory experiments, and our class-room management; but, apart from special pieces of apparatus planned for particular researches, we have not yet an adequate exchange for ideas concerning laboratory equipment. The annual exhibition has not induced enough laboratories to participate and not all new pieces from our laboratories can be safely and conveniently transported. However useful these annual displays are—and the author does not mean to disparage them in any way,—accurate descriptions of apparatus in our journals, accompanied by illustrations whenever possible, are sure to appeal to a larger group and will leave a more permanent record in our archives. Demonstrations, in our opinion, are invaluable, but they ought to be accompanied by accurate specifications. By writing out in detail a series of descriptions of new equipment designed and constructed in our laboratory during the last few years, the author attempts to express his agreement with Seashore when he writes:

> The Cornell Laboratory has set a good example in reporting detailed specifications for the following five pieces of apparatus. It is very desirable that the various laboratories should adopt the plan of describing fairly permanent pieces of apparatus apart from the report of the research.[1]

TUNING-FORK OF VARIABLE INTENSITY.

The principle upon which the construction of this apparatus rests is that of wave-interference and virtual cancellation at points lying in planes which make angles of 45 degrees with

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[1] Seashore, C. E. Apparatus, *Psychol. Bull.*, 10, 1913, 32.

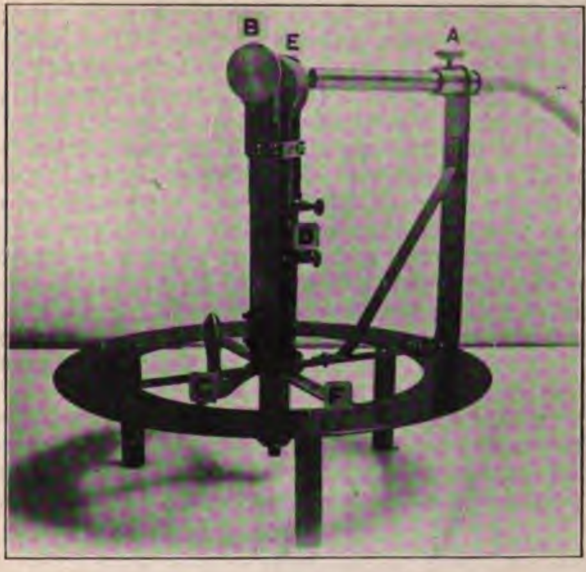

FIG. 1.—Tuning-Fork of Variable Intensity.

a plane passing midway between the tines of a tuning-fork and parallel to their sides. If the tines of a tuning-fork are therefore rotated opposite a conducting tube which is held stationary in a position perpendicular to the axis of rotation, a point of virtual cancellation of wave disturbance, together with phases of partial cancellation on either side, will pass the tube four times in a complete revolution.[2]

The fork is electromagnetically driven by means of a primary fork operated in series with it through the binding posts (D) and electromagnet (E), and removed to another room to eliminate extraneous noises. It is accurately tuned to a pitch of 100 vd. and the amplitude of its vibrations can be gauged to 1/1000 in. by means of a micrometer-screw (B) whose head, 1¼ in. in diameter, carries a scale indicating twenty-fifths of a revolution and whose stem, with 40 threads to the inch, permits the adjustment of its tip until contact is made between it and the side of the tine. A brass tube, 5 in.

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[2] A similar form of apparatus has been suggested by F. M. Urban: Ein Apparat zur Erzeugung schwacher Schallreize, *Arch. f. d. gesam. Psychol.*, 27, 1913, 232-234. The principle of interference is described in detail in Barton, E. H. A text-book on sound, 1908, 374-377; Helmholtz, H. L. F. On the sensations of tone (trans. Ellis), 3rd Eng. ed., 1895, 161; Windelband, A. Handbuch der Physik, Vol. 2 (Akustik), 1909, 602.

long and ½ in. in diameter, and a large rubber tube connected to it, conduct the sound to the ear. The brass tube is held in position by means of an angle-brace, is adjusted by rotating the knurled screw (A), and terminates on the side nearest the fork in a vertical slit ⅜ in. long and ⅛ in. wide.

The fork is mounted on a spindle to which a handle and indicator (C) are attached. On the circular base the degrees of rotation are engraved, starting with the zero-point at the position in which the tines of the fork are both in a straight line with the tube and extending 90 degrees on either side. Most of the construction is of 1/16 in. brass. The base is 10 in. in diameter; the brass tube stands 6¾ in. above the base; and the three legs are 2¾ in. long and are covered with rubber tubing which extends over their ends to diminish conduction of sound through the base.

The apparatus readily lends itself to experiments in the field of auditory acuity (*cf.* note 1), and if the intensity is accurately calculated from physical equations, it ought to be of service in investigations of the *DL* as well as the *RL* for intensity.

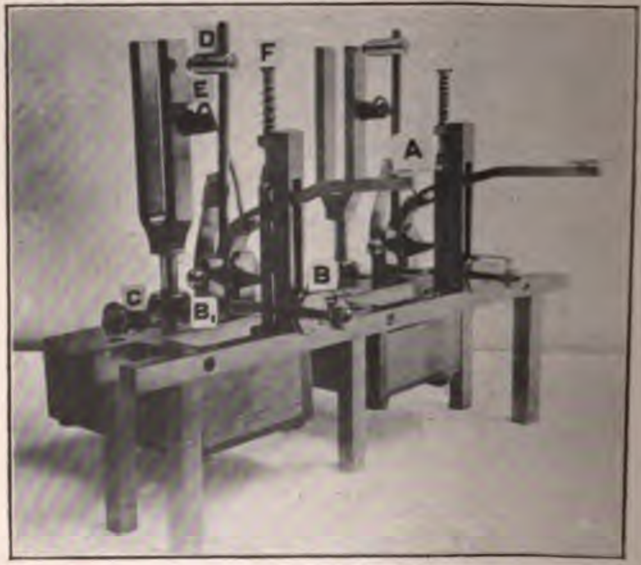

FIG. 2.—Automatic Tuning-Fork Hammers.

Since this apparatus is simply a double form of an instrument described in a previous article, detailed specifications will be omitted.[3] The type of apparatus illustrated has the advantage of being more easily adapted to experimental investigations in which two successive stimuli, differing in pitch or in intensity, are required at short intervals. The essential features are: a key-rod (A) which depresses a slide (F) and momentarily releases the damper (E) while the fork is being struck. The hammer-piece (D) is engaged by means of a pawl by the slide in its downward movement and against the action of a tension-spring (seen to the right of B), whose tension can be regulated by a knurled nut (B). Near the point of extreme depression the hammer-piece is suddenly and automatically released. It strikes the tine of the fork with a force dependent upon the tension of the spring attached to its lower end. A clamp (C) is provided for holding the fork in place and is adjustable to fork-stems of varying thicknesses. The instrument serves very well for demonstrational purposes in large lecture-rooms and for investigational studies in the laboratory.

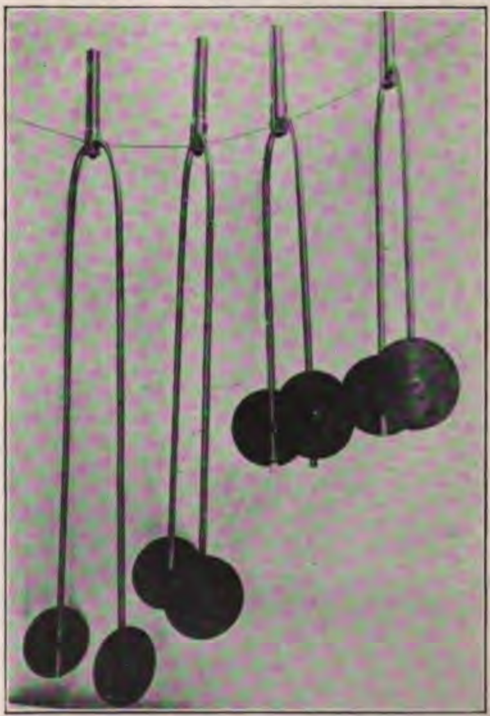

FIG. 3.—Adjustable Wire Forks.

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[3] Bentley, M., Boring, E. G., and Ruckmich, C. A. New apparatus for acoustical experiments, *Am. J. of Psychol.*, 23, 1912, 515-516.

ADJUSTABLE WIRE FORKS.

A set of four wire forks has been made for the purpose of investigating the stimulus-limen for pitch, the relation of noise to tone, and analogous problems in acoustics. The four forks are uniform in construction; dimensions differ only in the length of tines and in the amount of their separation at the extreme ends. The tines of the two smallest forks measure 17 in. in length and are separated 5½ in. at the end; the tines of the next larger fork are 24 in. long and are separated 6 in. at the end; and the largest fork has tines 29 in. long and separated 6½ in. at the end. The tines of all of the forks are slightly divergent. They are made of ⅜ in. tool-steel with a handle of ¾ in. steel cut to a length of 5¾ in. A heavy pin and screw fasten the tines to the handle. Hard fibre discs, 3/16 in. thick and 3⅞ in. in diameter, mounted on the tines by means of adjustable brass thumb-screws, serve as riders and at the same time amplify the intensity of the vibratory disturbance. When permanently adjusted and calibrated, the total range of the forks will extend from 8 to 40 vd.

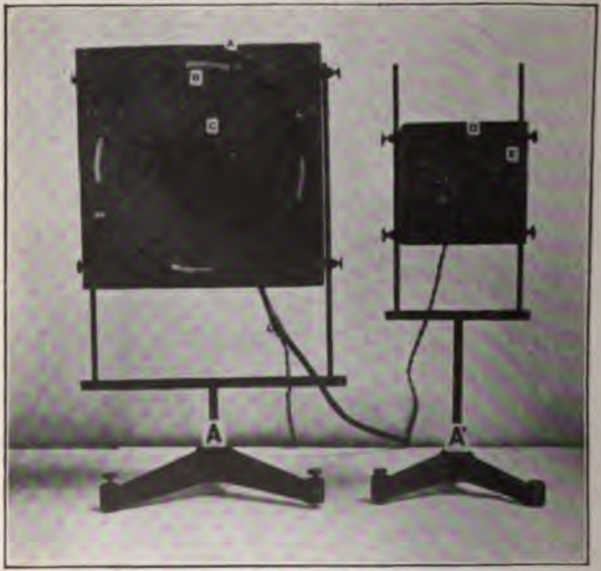

Fig. 4.—Blind-Spot Apparatus.

In connection with a doctorate thesis it became necessary to provide a reliable control of eye-movement. One of the methods used in this research was the well-known procedure involving successive determinations of the position of the blind-spot. There was no standard piece of apparatus available by means of which this work could be quickly and accurately done. The requirements of the method were satisfactorily met by instruments which were constructed in the departmental machine-shop.[4] The apparatus on the left (A) is used to map out the blind-spot; the one on the right (A′) furnishes the fixation-point. The experiment is performed under dark-adaptation. The principal feature of the larger apparatus is a sheet-iron box 12 in. square and 6 in. deep, mounted by a set of four thumb-screws on a bracket and tripod 22 in. high. The box contains a 16 c. p. electric incandescent lamp fixed in the center of the door which forms the back. The front of the box carries a circular plate 12 in. in diameter. This plate is attached to the box by four broad rivets moving in slots which are cut into the plate in the form of an arc of 45 degrees (one of these arcs is to be seen just above B). The plate is rotated through this arc by means of two projecting handles. Two circular rows of eight discs are mounted on the plate, those in the outer row (B) measuring 2 in. in diameter with centers 3 3/32 in. from the center of the plate, those in the inner row (C) measuring 1⅛ in. in diameter with centers 1½ in. away from the center of the plate. The discs are provided with projecting flaps by means of which they may be rotated. A series of five holes is drilled in each of the larger discs, and a series of three holes in each of the smaller discs. These holes are located on concentric circles with 3/16 in. difference in radius and at points 22½ degrees of rotation apart. The holes are 3/32 in. in diameter. Behind the discs, drilled into the plate along each of four partial diameters that are spaced at angles of 45 degrees, there is a series of 32 still smaller holes corresponding exactly to the successive positions of the holes in the discs under which they lie. The light within the box is allowed to penetrate the holes in the plate only when the holes in the discs are made to correspond with them. Since each disc may be rotated 360 degrees, it successively uncovers double the number of holes that it contains. Furthermore, since the large circular plate may be rotated 45 degrees and since the exact position of the plate may be determined from an engraved scale (barely seen to the right of small A), all points, located in a circular field of about 8 in. in diameter and at distances of 3/16 in. units from the center of the plate, can be investigated. The fixation-box (A′) in a like manner permits the adjustment of the fixation-point in the vertical and in the horizontal direction. The vertical direction is controlled by a sliding rod (D) and the horizontal direction by two discs 2½ in. in diameter. The holes are of the same diameter as those in the other box, are 3/16 in. apart, and number twelve in each direction in addition to the common central point. The box is also adjustable in the vertical plane by means of four set-screws on the side of the box; it is cubical in shape; and it measures 6 in. on each side. A 16 c. p. incandescent lamp is fastened inside the box. Both pieces of apparatus are finished in flat black on the outside and are enameled in white on the inside. They are provided with electric cord attachments for a 110 v. circuit; resistance may be introduced to avoid halos or pronounced after-images. Two set-screws are inserted in each tripod in order to give the boxes an absolutely vertical position.

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[4] While the suggestions of members of the department are in large measure responsible for the original design of pieces of apparatus described in this article, Mr. Clayton F. Harding, the departmental machinist, has added other suggestions or has materially improved the original design in the course of construction. The department will undertake to supply any of the pieces described at prices which can be had on application.

The apparatus is set up a meter and a half from the observer's eye. His head is kept steady in a head-rest. After the fixation has been determined, the blind-spot apparatus is adjusted so that points are found that are just within the blind area. The distance between the blind-spot apparatus and the fixation-point apparatus is arranged to suit the requirements of different observers. When the apparatus is set for any one individual, the points thus determined naturally mark the areal extent of that observer's blind-spot at the distance mentioned. Whenever any one or more of these points become visible, other conditions[5] being equal, eye-movement is indicated.

DISC CUTTER.

An improved form of cutter, used in connection with experiments in color-mixture, in peripheral vision, and in any field which requires paper-discs of uniform sizes, has been built in the laboratory and has given satisfactory service. While it was built primarily for cutting paper-discs, with the exercise of care, thin cardboard may also be cut.

The base (F) consists of ½ in. cast-iron covered with 1/16

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[5] These conditions include absolute fixation of the head, accurate and immediate report of the visibility of the spots of light, and the special factors which must be taken into account generally in investigations of the visual sensations.

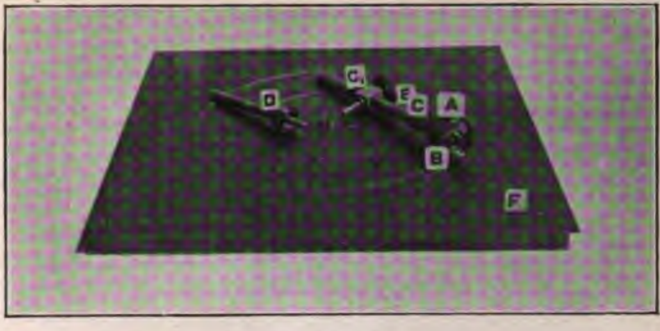

FIG. 5.—Disc Cutter.

in. soft brass which is fastened to the iron by means of six brass machine-screws. The base measures one foot square. The cutting-wheel (B) is made of tempered steel, one inch in diameter, and is protected by a knurled brass hood (A), ¾ in. wide. On the side which faces the base it is flattened to permit the projection of the blade on the surface of the paper. The hood is permanently screwed to the stem of the cutter to prevent rotation and consequent tearing of the paper. The pivot (E) is made of steel, 9/16 in. square, and contains on its lower surface a steel pin, ⅝ in. long and 3/16 in. in diameter, which fits into a steel die, mounted in the center of the base, measuring ½ in. in outside diameter, and projecting 3/16 in. above the surface of the base. On the upper surface of the pivot, a knurled brass thumb-screw is provided for adjusting the distance between the pivot and the cutting-wheel. This thumb-screw is about an inch in diameter and carries a pin which fits into a key-way on the upper surface of the cutting-stem. This also insures against the rotation of the hood (A). For purposes of rapid and accurate adjustment, two stops (C and C_1) are provided. These consist of brass collars that fit the cutting-stem. They are fastened with small knurled thumb-screws which project into the key-way on the stem. The two positions at which these stops are maintained correspond to the two sizes of discs commonly used in our laboratory, *viz.*, 11 cm. and 20 cm. in diameter respectively. The stem is graduated in centimeters to facilitate the process of adjustment. A center-punch (D) completes the outfit. The pointed end of this device fits into the steel die in the center of the base and the brass cup which is mounted on the stem is made just large enough to pass snugly around the outside of the die.

The cutting operations are as follows: (1) the sheet from which the desired disc is to be cut is placed on the base so that the circle, engraved thereon from previous cutting and therefore representing the desired size, is just within the limits of the sheet; (2) the position of the center-die is determined by a slight pressure with the thumb on that portion of the sheet; (3) the centre-punch is brought point down into the die to cut the hole and is then removed; (4) the cutting-tool, with its wheel adjusted at the proper cutting distance from the pivot, is inserted into the hole previously made, and is then revolved over the surface of the sheet.

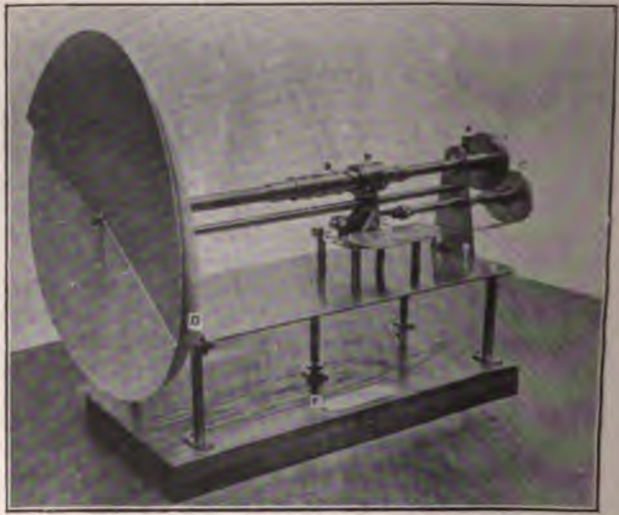

Fig. 6.—Variable Color-mixer.

This is a modification and enlargement of the Musil[6] color-mixer which, in turn, is an improved form of the Marbe mixer. With the larger construction naturally new problems of detail had to be solved, so while the principle is not entirely new, several of the important features are original. Reference to the diagrams which illustrate the Musil pattern will indicate the major changes.

The colored papers which represent the color-components to be mixed are mounted on cardboard to prevent ruffling and tearing. The diameter of the discs which the apparatus will carry is 16 in. The discs are slit in the usual way. On the flap nearest the machine a cardboard lip is attached and is inserted through a slit in the heavy backing behind the color-discs. An ordinary pin holds this securely in place. The backing and the attached color-disc are mounted on the inner axle of the machine. The other color-disc is attached by means of a clamp (D) to a balanced arm which projects from the outer axle or casing. Except when changes are made in the composition of the mixture, both inner and outer axles are rotated together with the pulley and shaft (A). This pulley is of brass, 3¼ in. in diameter; the shaft with its attachments is 23 in. in length. The steel inner axle is ⅝ in. in diameter; the outer brass casing is ¾ in. in diameter. Oil-cups and bushings are provided at both ends and at the end nearest the color-discs where the wear is greater, ball-bearings are located. To vary the mixture the large brass dial (C) is turned. The amount of change can be read on the face of the dial in units of a degree or fraction thereof. For convenience and accuracy a pointer indicates the exact position of the dial. As the dial is rotated, the steel stem, to which it is attached and which carries 11 threads to the inch, moves the carriage (behind C_1) along tracks on the platform (on either side of C_{11}). The carriage-arm is flanked by two ball-bearing collars (B) which are fastened to the casing containing the spiral key-way. The key-way makes one complete turn in 3½ in. of its length and is reënforced by three brass bands which encircle it. To prevent this casing from turning in relation to the inner axle, it carries a pin which travels in a straight key-way cut into the inner steel axle (between A and B). The outer axle with its casing attached to the variable color-disc is equipped with a lozenge-shaped pin which follows the spiral key-way. Since the casing which carries the spiral key-way cannot rotate with respect to the inner axle and its color-disc, the outer axle with its variable color-disc and casing must be the one to rotate in relation to the inner axle when adjustment is being made, because the pin which guides its movement is compelled to follow the spiral path as the casing which contains this spiral key-way is moved back and forth by the carriage-arm. By means of a pointer attached to the carriage, the platform (C_1) indicates samples of the colors which are being mixed and the respective amounts of the mixture in units of five degrees. The whole apparatus stands about a foot high on an oak base, 10 in. wide and 20 in. long, and is supported by five legs, the center one (F) being adjustable to prevent the sagging of the large brass base-plate. The apparatus has been tested out with a 1/6 H.P. Emerson electric motor at a speed sufficiently rapid to prevent flicker and has been found serviceable for demonstrational purposes.[7]

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[6] Spindler and Hoyer, Psychological apparatus, Catalog No. 21, 1908, pp. 52-54.

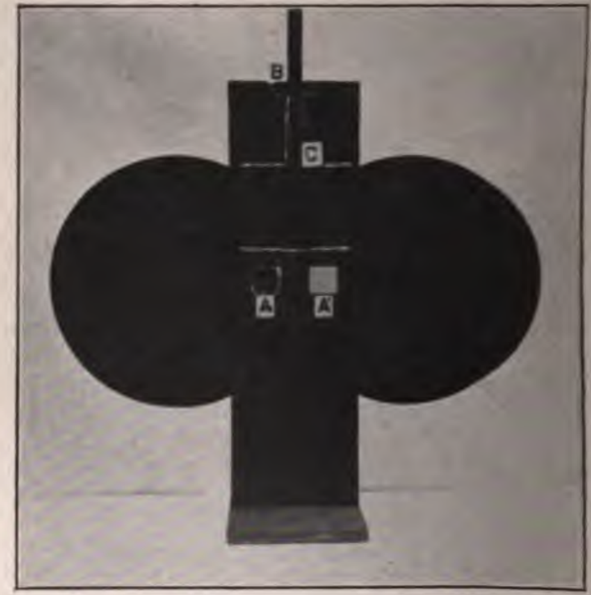

FIG. 7.—Apparatus for Paired Exposures.
(Front View)

This apparatus was built for the purpose of presenting a series of twelve colors according to the procedure of paired comparisons in the standardised form of the method of impression. It is used both for laboratory exercises and for demonstrational work.

On the side facing the observer (fig. 7) nothing is seen except the upright wooden frame 30 in. high and 9 in. wide at the base with two partial circles extending 12 in. on either side of the upright frame. When used in laboratory exercises, the experimenter raises the slide (fig. 7, B) and with it the attached shutter (fig. 7, just below C) by means of the handle (fig. 8, under B). If he finds it convenient, the

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[7] The type of motor used automatically assumes the load only after its full speed has been attained.

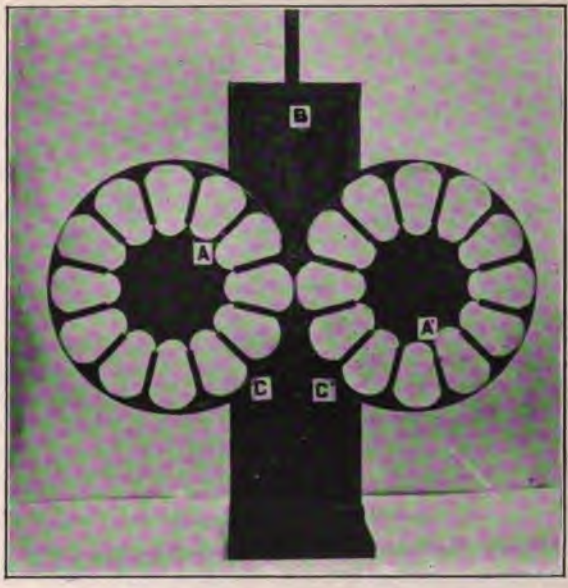

Fig. 8.—Apparatus for Paired Exposures.
(Rear View)

experimenter may set the pivoted end of the slide into a metal socket provided for the purpose of keeping the shutter open. The colored papers are then visible through the two apertures (fig. 7, A and A′), which are 2 in. square and 2 in. apart. The colors are changed by rotating the sheet-iron wheels (fig. 8, A and A′), which carry the papers in thin metal runways. A backing of cardboard serves to keep the papers flat and displays the number of the color in the series. Holes, 2¾ in. in diameter, are cut into the wheels opposite the apertures seen from the front (fig. 7, A and A′). To make certain that the colors are in their correct positions, notches are cut in the circumferences of the wheels to correspond to each successive exposure. A spring trigger (fig. 8, barely seen at C and C′) momentarily prevents further rotation. The apparatus is finished in dead-black. It is our experience that by means of this apparatus the papers are presented in a much more systematic fashion and are kept in better condition than in the process of ordinary manipulation.

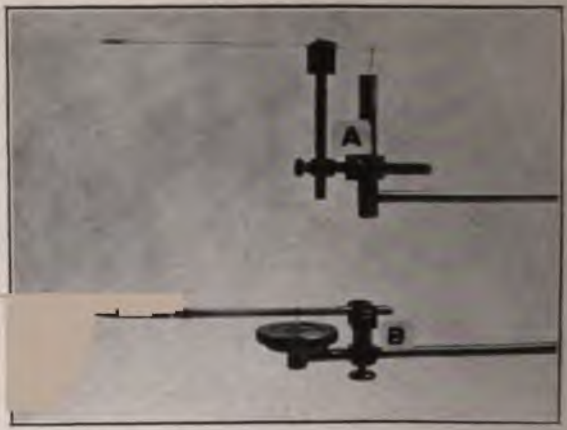

Fig. 9A.—Metal Piston Recorder.
Fig. 9B.—Demountable Tambour.

The principal improvement in this form of recorder lies in the fact that it is constructed entirely of metal; the pivots are of tempered steel, the recording arm is of aluminum, and the rest is of brass. Hence it has an advantage over the Lombard-Pillsbury form in that the irregularities of plaster and glass are avoided.[8] The cylinder, measuring 3 in. in length and ¼ in. in inside diameter, is carefully bored and ground out and accurately fits a small brass plate, less than 1/32 in. in thickness, which forms the piston-head. Into this plate is soldered a very light brass wire, 1½ in. long, and hooked at the end which enters the hole in the aluminum recording arm. This arm, 8 in. in length, is accurately balanced and carries on its farther end a short hog's bristle. The entire moving parts weigh 1.542 grams, or about four times the weight of the corresponding parts in the Lombard-Pillsbury recorder. Two small set-screws are provided to hold the pivots in position; the pivots are also adjustable.

While we have not yet tested the inertia and comparative accuracy of the recorder, several score trials have resulted in records that compare favorably with those made by the earlier models. The apparatus requires much less attention. For one thing, the need of constant lubrication and the consequent removal of accumulated oil is avoided. It is likely that further improvement will be made along the line of reducing the weight of the moving parts.

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[8] Lombard, W. P., and Pillsbury, W. B. *Am. J. of Physiol*, 3, 1900, 186-200. Modifications of this form of recorder are described in Shepard, J. F., The circulation and sleep, 1914, p. 7.

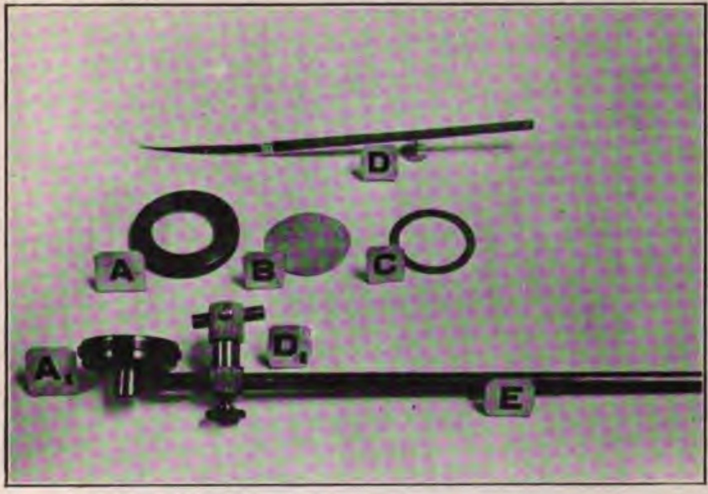

FIG. 11.—Demountable Tambour.

The chief advantage in this form of Marey tambour lies in the fact that the rubber dam is very readily renewed. The tambour is shown in both the assembled (fig. 9B) and in the unassembled state (fig. 11). The main parts are: (1) the writing-arm (D), 6½ in. long, made of an ordinary soda-straw, with a celluloid point attached by means of an aluminum band and with a bent celluloid bridge which rests on the rubber dam; (2) a knurled brass cap (A) which, after the rubber dam (B) and the washer (C) have been put into position, fits over the capsule (A_1) and holds the dam tightly over the capsule with an area of about an inch in diameter left free to vibrate; and (3) the stem (E) made of brass tubing 9½ in. long and ¼ in. in diameter, opening into the capsule and carrying the pivot (D_1) into which the writing-arm is inserted (as in fig. 9B). A half-inch knurled set-screw attaches the pivot-block to the stem and allows adjustment for writing-points of various lengths. Since the bridge is also adjustable, the same writing-point may be given greater or less length and consequent excursion on the drum.

ADJUSTABLE STANDARD.

A new pattern of adjustable standard has been constructed in the laboratory. The main adjustments are made by means of a thumb-screw (B), one inch in diameter, and a knurled nut (A), 1½ in. in diameter. The thumb-screw has a long

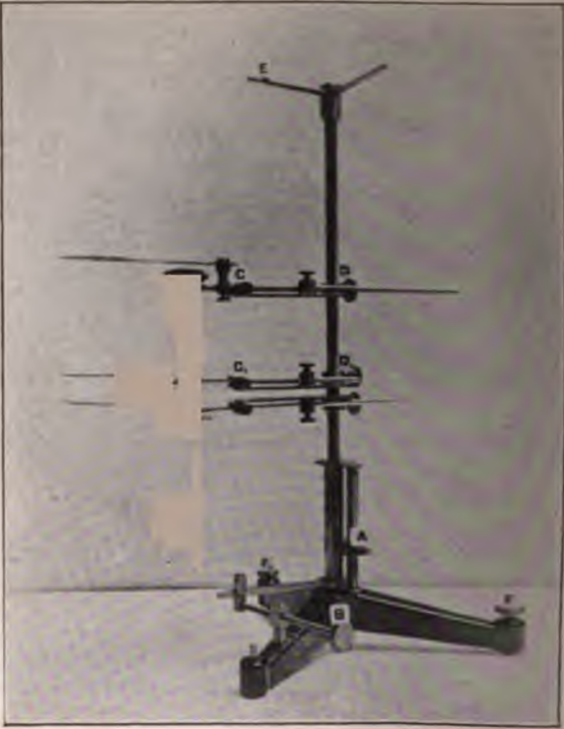

FIG. 10.—Adjustable Standard.

threaded stem which passes through a horizontal pivot-block inserted into one of the legs of the large cast-iron tripod. The stem has a total length of 5½ in. and a diameter of ¼ in. and carries twenty threads to the inch. On the opposite end from the head this stem is fitted into an arm projecting 5 in. from the center of the main shaft and fastened thereto. The shaft rotates on its vertical axis in obedience to the adjustments of the thumb-screw (B), and consists essentially of two parts: the upper, made of half-inch solid brass rod and telescoping into the lower in response to adjustments made by means of the knurled nut (A). The latter moves vertically on a stem, 4½ in. long and ½ in. in diameter, carrying 12 threads to the inch. In so doing it operates a ¼ in. pin with a roller bearing which travels in a groove deeply into the side of the nut. This pin is rigidly fastened i the telescoped portion of the upper shaft. A key-way is c into the side of the lower shaft facing the nut to allow the pin to move with the nut. The lower shaft is made of ¾ in. brass. The threaded stem is held in place by two horizontal projections 1½ in. from the center of the main shaft. As the nut is rotated, it moves up or down the stem and carries with it the upper portion of the standard to which the markers are attached.

Three arms are provided with the standard, two of them so arranged that they may be moved as closely together as the instruments carried will permit. The arms are constructed of ⅛ in. brass, as are all similar parts in the apparatus; they vary from 2¼ to ⅞ in. in width, and are 4⅝ in. long; and they are fastened to the upper shaft of the standard by ⅞ in. set-screws which travel in a key-way cut in the shaft (D and D_1). A half-inch collar is attached to the arm to give sufficient body to the thread for the set-screws. A block is provided for attaching the markers or writing devices. This is one-half inch high and ¾ in. in diameter, and has a ⅜ in. hole drilled horizontally through it. The required device is inserted into this hole, is fastened by means of the set-screw inserted from the top, and is permitted side-play by virtue of the slight rotation of the block on the arm. At the outer end of the arm (C and C_1) is a set-screw, 1½ in. in length, which carries a loose concave plate at the end of its stem. Operating against this plate is a similar one backed by a compression spring on a sliding rod, 1½ in. long. The writing device is inserted between these plates and can be adjusted toward or away from the kymograph-drum by turning the set-screw.

At the top of the shaft, two additional legs are attached so that the standard may also be used in the horizontal position. These legs are 4¼ in. long, ¼ in. in diameter, and are attached to a movable sleeve at an angle of about 60 degrees with one another. The standard is made almost entirely of brass, stands at an average height of 24 in., and can be adjusted in the vertical position by two set-screws mounted in the tripod (F and F_1).

WALL-CHARTS

A series of twenty-five charts, appropriate for use in the lecture-room and in the laboratory, has been prepared under the direction of the department. The charts were made by means of a compressed air-brush in the hands of the University Artist.[9] About one-half of them are tinted in colors;

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[9] Requests for further information concerning these charts may be addressed to the Department of Psychology, University of Illinois, or to the artist, Mr. Charles W. Redwood, Forestry Bldg., Cornell University.

the rest are in black and white. The material used was water-color on heavy 'elephant paper' with cloth backing, 42 in. in width and from three to five feet in length. They were prepared ready for hanging with a strip of molding on each end.

Appended is a list of these charts; they are numbered in Roman notation. The Arabic numbers refer to the separate figures on the chart. There is a brief description and a citation of the source from which the chart was copied in each instance.

I. Organ of Audition.
  1. Semicircular canals in cochlea.
    From a stereopticon slide: photographic reproduction of a plaster model.
  2. Modified diagrammatic transection of right ear.
    After Czermak.
  3. Diagrammatic transection of a whorl of the cochlea.
    Foster, M. A text-book of physiology, 1891, vol. 4, fig. 177, p. 203.

II. Organ of Corti.
  1. General transection of the organ of Corti in the cochlea of a new-born pig.
    Shambaugh, G. E. *Arch. of Otol.*, 37, 1908, plate xiv, fig. 2, opp. p. 457.
  2. Similar transection showing bloodvessel under basilar membrane.
    Ibid., fig. 4.
  3. Similar transection to show the attachment of the membrana tectoria to hair-cells.
    Shambaugh, G. E. *Zeits. f. Ohrenhlk.*, 62, 1910, Taf. x, Abb. 2 (reversed), opp. p. 235.
  4. Transection similar to 3, to show attachment to outer hair-cells.
    Ibid., Abb. 5.

III. Analyses of complex Sound-waves.
  1. Graphic analysis of irregular, periodic, longitudinal vibrations into simple, regular, periodic, sinusoid vibrations.
    Wundt, W. Grundzüge der physiologischen Psychologie, 6th ed., 1910, vol. 2, fig. 171, p. 70.
  2. Graphic analysis of complex sound-wave into waves corresponding to a simple tone with an additional simple tone of double vibration-rate and of less intensity.
    Ibid., fig. 172(A), p. 72.
  3. Graphic analysis of complex sound-wave into waves corresponding to a simple tone with an additional simple tone of triple vibration-rate and of less intensity.
    Ibid., fig. 172(B), p. 72.

IV. Graphic representation of Vocal Sounds, showing irregular wave-structure.
  1. German vowel ā at c (= 128 vd.)
    Ibid., fig. 245(1), p. 396, after L. Hermann.
  2. German vowel ō at c (= 128 vd.)
    Ibid., fig. 245(2), p. 396, after L. Hermann.

3. German vowel ū at c (= 128 vd.)
   Ibid., fig. 245(3), p. 396, after L. Hermann.
4. German vowel ē at c (= 128 vd.)
   Ibid., fig. 245(4), p. 396, after L. Hermann.
5. German vowel ī at g (= 192 vd.)
   Ibid., fig. 245(5), p. 396, after L. Hermann.
6. German pronunciation of 'ibba' at g (= 192 vd.)
   Ibid., fig. 246(7), p. 399, after L. Hermann.
7. German pronunciation of 'ikki' at e (= 160 vd.)
   Ibid., fig. 246(8), p. 399, after L. Hermann.

V. Difference Tones.
   Schema of the primary difference tones with reference to the interval of the generating tones within the octave.
   Krüger, F. *Arch. f. d. gesam. Psychol.*, 1, 1903, 272.

VI. Circle of Complementary Colors.
   Hering's theoretical schema of the four primary colors and their resultant mixtures.
   Hering, E. Lehre vom Grundzüge der Lichtsinn, 1905, opp. p. 42; also in Graefe-Saemisch Handbuch, 2nd ed., pt. 1, chap. 12, and in Myers, C. S. A text-book of experimental psychology, 2nd ed., 1911, pt. 1, plate i, fig. A, opp. p. 89.

VII. The Retina and Visual Theory.
1. Section of the retina.
   Lickley, J. B. The nervous system, 1912, p. 92.
2. Graphic representation of the Hering components of color-vision as distributed over the visual spectrum.
   Ibid., p. 99.

VIII. The alleged migration of Retinal Pigments.
1. Distribution of pigment-bodies during light-stimulation.
   Nagel, W. Handbuch d. Physiologie d. Menschen, 1905, vol. 3, Taf. i, opp. p. 94.
2. Distribution of pigment-bodies before light-stimulation.
   Nagel, W. Handbuch d. Physiologie d. Menschen, 1905, vol. 3, Taf. i, opp. p. 94.

IX. Miscellaneous Organs of Sense.
1. Olfactory receptor-organ in left nasal cavity.
   Lickley, J. B. *op. cit.*, p. 113.
2. Olfactory receptor-cells.
   Ibid., p. 113.
3. Longitudinal and transverse section of a Meissner corpuscle in cutaneous tissue of the hand (cutaneous pressure).
   Ibid., p. 117.
4. Gustatory organs in papilla foliata.
   Ibid., p. 115.
5. Gustatory organs in gustatory pore and taste-bud.
   Ibid., p. 115.
6. End-bulb of Krause in cutaneous tissue (cold).
   Ibid., p. 117.
7. Pacinian corpuscle (subcutaneous or muscular pressure).
   Ibid., p. 118.
8. Nerve-ending of Ruffini (warmth).
   Ibid., p. 117.
9. Spindle of Golgi in tendon (tendinous strain).
   Ibid., p. 119.

X. General view of demonstrational **Rhythm-hammer** (Cornell pattern).
   Illustrating device for demonstrating temporal perceptions.
      Bentley, M. *Am. J. of Psychol.*, 23, 1912, fig. 4, opp. p. 514.

XI. Cathedral Perspective.
   Shows architectural curve in pillar to produce illusory exaggeration of height as viewed from the floor.
      Goodyear, W. H. *Memoirs of Art and Archaeology* (Museum of Brooklyn Inst. of Art and Sciences), 1894, vol. 1, no. 4, p. 26.

XII. Dioptrics of the Stereoscope.
   A dioptric diagram in colors illustrating the principle of the Brewster stereoscope.
      Simplified from Titchener, E. B. Experimental psychology, 1901, vol. 1, pt. 2 (Instructor's manual), fig. 66, p. 268.

XIII. Synaesthesia: chromaesthesia.
   Illustrations representing vowel, consonant, number, and word associations with colors.
      Galton, F. Inquiries into human faculty, etc., 1883, plate iv, fig. 69, after p. 380.

XIV. Synaesthesia: number-forms.
   Illustrating the association of numerical series with diagrams.
      Galton, F. *Op. cit.*, plates i, ii, iii, figs. 1. 2, 7, 8, 9, 10, 20, 27, 37, 56, 59, after p. 380.

XV. The Expressive Method as applied to Emotion.
   Fig. 1-3. Sthenic type of emotion expressed by curves representing breathing (above) and blood-volume, pulse-rate, and Traube-Hering waves (below).
   1. Transition from pleasurable thought of successful work to unpleasurable thought of fatigue at 'a'.
      Wundt, W. *Op. cit.*, 5th ed., 1903, vol. 3, p. 228.
   2-3. A continuous curve representing strong anger induced by memory of heated argument. At first pulse is depressed (unpleasantness). Depression increases with continued emotion. Breathing is strengthened, shortened, and irregular.
      Ibid., p. 229.
   4. A curve representing momentary and depressing emotion of sudden fright. Decrease of volume and pulse, and irregular breathing.
      Ibid., p. 230.

XVI. Plan of Reaction Experiment.
   Disposition of apparatus and electrical connections in the typical simple reaction experiment requiring the Hipp chronoscope and accessory instruments.
      Wundt, W. *Op. cit.*, 6th ed., 1911, vol. 3, fig. 368, p. 366.

XVII. The Human Brain.
   1. Adult type of fissuration, female, 29 yr., dorsal aspect, rich in gyres.
      Retzius, G. Das Menschenhirn, 1896, vol. 2, plate 54, fig. 2.
   2. Adult type of fissuration, male, 37 yr., dorsal aspect, poor in gyres, but unusual in type.
      Ibid., plate 54, fig. 1.

3. Ventral aspect, showing cerebellum, male, 48 yr.
   Ibid., plate 60, fig. 1.
4. Lateral aspect of left hemisphere, showing left aspect, male, 36 yr.
   Ibid., plate 64, fig. 1.
5. Median section of cerebrum, showing median aspect of left hemisphere, male, 50 yr.
   Ibid., plate 67, fig. 2.

XVIII. The Earthworm.
   1. General view.
      Parker & Haswell, Text-book of zoölogy, 1910, vol. I, p. 454.
   2. Anterior segments.
      Ibid., p. 454.
   3. Sagittal section through anterior half.
      Ibid., p. 457.
   4. Central nervous system.
      Ibid., p. 459.

XIX. Internal view of Earthworm.
   Stereographic representation of the circulatory, digestive, and nervous systems in the middle segments (in color).
      Original sketch by Redwood, *et al.*

XX. The Nervous System of the Earthworm.
   1. Longitudinal section showing peripheral nervous system: cord and setae.
      Lenhossék, M. von, *Arch. f. mikros. Anat.*, 39, 1892, Taf. v, fig. 6.
   2. Free nerve-endings in epidermis.
      After Smirnow.
   3. Transection of cord showing neuronic connections.
      Lenhossék, M. von, *op. cit.*, Taf. v, fig. 10.

XXI. Sense-organs of the Earthworm.
   1-2. Visual cells.
      Hesse, R., *Zeits. f. wiss. Zoöl.*, 61, 1896, Taf. xx, fig. 6b, 14.
   3-6. Sense-organs in epidermis (chemical).
      Langdon, F. E., *J. of Morph.*, 11, 1905, plate xiii, fig. 1, 4, 11, 2, opp. p. 232.

XXII. Discrimination-box (University of Illinois model).
   Cf. Watson, J. B., and Yerkes, R. M., Methods of studying vision in animals, *J. of Animal Behav., Monog. Supp.*, I, no. 2, 1911, 18.
   1-3. Showing side, top, and general views, respectively.

XXIII. The Animal Maze (University of Illinois standardisation).
   General top view of the model, 5 ft. 2 in. in diameter, showing detention-box and wire-cover in additional diagrams.

XXIV. Ant-nest (University of Illinois modification of the Janet-Fielde nest).
   Combined top and side view of the plaster-model, 16¼ in. x 5¾ in., showing the four compartments and openings for the extraction or insertion of ants.

XXV. The Salivary Reaction of the Dog (Pavlov's method).
   Diagrammatic representation of the disposition of apparatus in connection with the expressive method as applied to the dog.
      Yerkes, R. M., and Morgulius, S., *Psychol. Bull.*, 1909, 6, 264. *V.* also Watson, J. B., Behavior, 1914, p. 66 (after Nicolai).

NOTES ON PRACTICE, IMPROVABILITY, AND THE CURVE OF WORK

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Teachers College, Columbia University

Learning and the Curve of Work in the Case of a Clerical Task

Sixty-four educated adults practiced writing the products of 11 x 11, 11 x 12, 12 x 12, 11 x 13, ... 19 x 19 with the aid of a key as shown below.

	11	12	13	14	15	16	17	18	19
11	121	132	143	154	165	176	187	198	209
12	...	144	156	168	180	192	204	216	228
13	...	...	169	182	195	208	221	234	247
14	...	...	...	196	210	224	238	252	266
15	...	...	...	...	225	240	255	270	285
16	...	...	...	...	...	256	272	288	304
17	....		...	...	...	...	289	306	323
18	...	...	...	...	...	...	...	324	342
19	...	...	...	...	...	...	...	...	361

The subject wrote as rapidly as possible the products of 80 pairs of numbers printed in a haphazard order as shown below, using the Key or trusting to memory. The time of beginning and ending and the number of mistakes were recorded. The time record was as exact as the subject could make it, using an ordinary watch with a second hand.

17 19
12 11
14 16
19 11
18 18
15 11
16 18
13 19
12 17

18 19
15 18
16 19
18 12
12 13
17 18
11 12
19 18
15 14

11 16
14 17
15 19
16 17
15 16
18 14
13 16
17 12
11 13

17 17
12 18
13 15
16 12
18 15
19 16
16 13
14 18
16 16

12 12
13 14
14 19
15 13
16 11
18 13
11 17
17 14
13 11

13	13	19	13	14	15	15	17	18	16
18	11	14	11	19	19	16	14	14	14
19	17	11	19	16	15	14	13	15	12
15	15	17	15	19	14	18	17	17	16
19	12	13	18	13	17	12	19	11	14
12	14	14	12	12	15	17	13	13	12
17	11	19	15	11	18	11	15	12	16

Four different sheets, of 80 pairs each, were used in rotation, each one twelve times, making forty-eight sheets done or 3840 entries. The distribution of the practice differed in different groups of subjects, being according to the following scheme:

	Number of sheets done per sitting	Number of sheets done per day	Number of subjects
The 2.2 group	2	2	9
" 2.4 "	2	4	11
" 2.8 "	2	8	10
" 2.8_2 "	2	8 every other day	10
" 8.8 "	8	8	9
" 8.8_2 "	8	8 every other day	15

The practice was preceded and followed by a test with a still different sheet of 50 pairs.

THE NATURE AND AMOUNT OF IMPROVEMENT

The improvement consisted in a mixture of memorizing the products, complete or partial, becoming acquainted with the arrangement of the key and skillful in using it, facility in reading the pairs, facility in entering the products and facility in what may be called "overlapping"—perceiving the next pair and beginning to look at the key or think of the remembered product while entering the product for the previous pair. In no case did the learning progress to complete and perfect memorizing of all the 45 products. No such case was reported, and the shortest times (1.33 sec. and 1.45 sec. per pair) are too long for perfect memorizing.

The improvement is, as would be expected, universal and large. The central tendency is to do twice as much per unit of time and with half as many errors, in the last four sheets of the forty-eight as in the first four. The gain from the preliminary test with 50 pairs to the final test is still greater.

THE EFFECT OF THE DISTRIBUTION OF TIME UPON IMPROVEMENT

In order to compare the effect of the different distributions of practice it is necessary to equalize the initial ability of the groups by subtracting one or more cases. When this is done, we have, as average scores with the first four sheets, averag scores with the last four sheets, average gross gains an average corrected gains, the following:

	Average score with sheets 1-4		Average score with sheets 45-48		Average gross gain		Average correcte gain, counting each erro as 10 seconds
	Secs.	Er'rs.	Secs.	Er'rs.	Secs.	Er'rs.	
2.2 (n 9)	1506	4.66	733	2.00	773	2.66	800
2.4 (n 10)	1531	4.10	750	3.20	781	.90	790
2.8 (n 7)	1523	3.57	809	3.57	714	00	714
2.8_2 (n 8)	1478	7.75	814	1.88	664	5.87	723
8.8 (n 9)	1492	8.22	764	3.22	728	5.00	778
8.8_2 (n 15)	1517	4.13	760	2.40	757	1.73	774

Taking these results at their face value, they show (1) that when 8 sheets are done daily or every other day, they are more profitably done 8 at a sitting than 2 at a sitting, (2) that whether practice is daily or every other day makes little or no difference in the improvement per unit of time spent, and (3) that whether practice is spread over 24 days or consolidated into 6 makes little difference provided the long day's work is done at one sitting.

The individual differences in improvement are, however, very large and the correction for errors is rather arbitrary, so that the quantities should be considered in combination with similar quantities found in other studies rather than alone. So considered, they seem to support the doctrine that in such minor clerical tasks as adding, substitution tests, hunting for items and the like, there is little or no advantage in very short periods of learning, but is some advantage in fairly long intervals between practice, other things being equal.

INDIVIDUAL DIFFERENCES IN IMPROVEMENT

Individual differences in improvement are large. The extremes for the total group are represented by (1) individual H. G. C., who worked the first four sheets of the forty-eight at a rate of 18.7 pairs per minute, and the last five sheets at a rate of 45.0 pairs per minute, spending in all 6967 sec., or 6237 sec. from the mid-point of the first four to the mid-point of the last four, and (2) individual Whi., who worked the first four at a rate of 7.93 pairs per minute, and the fourth four at a rate of 9.6 pairs per minute, spending so far 8440 sec., or 6230 sec. from the mid-point of the first four to the mid-point of the fourth four. The use of the four sh average is a little unfair to Whi., but this is balanced by fact that his errors increased from 0 to 9 whereas those of H. G. C. decreased from 3 to 1. H. G. C. and Whi. then, from equal amounts of time given to practice, made gains of 26.3 and 1.7 respectively, in the product produced per unit of time.

The rate of gross gain in the product produced per unit of time is, as I have elsewhere shown, positively correlated with initial ability.

The facts for groups of high and low initial ability are shown below.

	Average number of minutes from beginning to end of the practice measured here	Average number of pairs per minute in 4 sheets	Average number of pairs per minute in 4 sheets done after about 122 minutes of practice (from mid-point to mid-point)	Gain in pairs per minute	Average number of errors in first four	Average number of errors in late four
8 initially highest individuals.......	134½	19.4	34.6	15.2	3.38	4.63
4 initially next highest individuals...	146	16.3	29.9(131 min.)	13.6	5.5	2.8
7 initially next to lowest individuals.	161	11.4	20.8(133 min.)	9.4	5.14	3.71
5 initially lowest individuals.........	155½	7.87	12.56	4.7	8.5	5.6

The effect of equalizing opportunity is thus to increase individual differences. This result, now found with many different functions, furnishes perhaps the strongest argument in support of the view that differences in achievement are largely due to differences in original capacity.

The correlation between initial ability and improvement is, of course, not perfect, fairly large differences in the latter being found amongst those of equal initial scores. Thus Br. and H. G. C., beginning at 18.5 and 18.6 pairs per minute respectively, gained 13.5 and 26.3, though the former spent more time than the latter. Thus Me. and Ch., beginning at 16.6 and 16.3, gained 7.0 and 20.8 respectively, though the former spent much more time than the latter.

CHANGES IN THE RATE OF IMPROVEMENT

The form of the practice curves in so short an experiment as this is not of much theoretical importance. It is in general of a type beginning with notable negative acceleration and passing thence into an approximately straight line, the form being roughly as in Fig. 1.

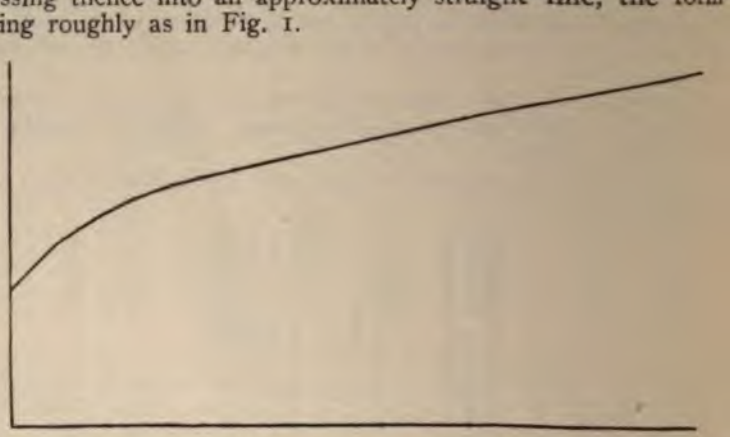

FIG 1.—The approximate form of the curve of practice in recording the products of pairs of numbers from a key.

THE CORRELATION BETWEEN SPEED AND ACCURACY

The total time spent and the total number of wrong products entered for each individual in 12 selected sheets are shown in Table I. This correlation table shows that there is a positive relation, and that in particular, no one of the twelve most rapid workers was amongst the dozen or so most inaccurate workers. The correlation between speed and accuracy by the formula r = cosine π U where U is the estimated percentage of the unlike-signed pairs, is somewhat over +.4.

THE CURVE OF WORK

We may use the records for those individuals who did eight sheets at a sitting for an analysis of the curve of work. In this I have utilized only the records of the last thirty-six or twenty-four sheets, so as to examine the function after it is well established.

When no allowance for errors is made, the average time required for the successive sheets of a sitting stood in the following relation: 108.3, 104.1, 99.5, 101.3, 98.7, 98.2, 96.2, 93.8. The P. E.'s (or median probable divergences of these quantities from the similar quantities obtained from an infinite number of similar experiments) are, in order: 1.5, 1.9, .9, .5, .5, 1.3, 1.3, .8. When 10 seconds is added for each

TABLE I

Correlation Between Speed and Accuracy

Time required for 960 products: In minutes: 39 = 39.0 to 41.0, 41 = 41.0 to 43.0, etc.

Errors made in 960 products	39	41	43	45	47	49	51	53	55	57	59	61	63	65	67	69	71	73	75	77	79	81	83	85	87	89	91	93
1– 3	..	..	..	1	..	..	..	1	..	1	1	..	1	1	..	1	..	..	..	..	..	..	..	..	..	..	..	..
4– 6	..	..	1	..	..	1	1	1	1	3	1	..	1	..	..	1	2	..	..	..	..	..	..	1	..	..	..	1
7– 9	..	..	1	..	1	2	1	2	..	2	1	2	1	..	1	..	1	..	..	1	..	..	..	..	..	..	..	..
10–12	..	..	..	..	..	..	..	2	1	4	..	1	..	2	2	1	..	..	1	..	..	..	..	..	..	..	..	..
13–15	1	..	..	1	1	1	1	1	2	..	..	2	..	1	4	3	..	..	1	..	1	..	..	1	..	..	..	..
16–18	..	..	..	1	..	..	1	1	..	1	1	..	2	1	1	..	1	..	..	..	..	..	..	..	..	..	..	..
19–21	..	..	..	..	..	..	2	1	..	..	..	1	2	1	..	..	..	..	..	1	..	1	1	..	..	..	..	..
22–24	..	..	..	..	1	..	..	1	1	..	2	..	1	1	2	..	..	..	..	1	..	..	..	..	..	..	..	..
25–27	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	1	1	..	1	..	..	..	..	..	..	..	..	..
28–30	..	..	..	..	..	..	1	..	..	..	..	..	1	..	1	..	..	2	..	..	..	..	..	..	..	..	..	..
31–33	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..	..	..	..	..	..	..	..	..	..
34–36	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1
37–39	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..	1	..	..	..	..	..	..	..	..	..	..	..	..
40–42	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..
43–45	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	1	..	..	..	..	..	..	..	..	..	..	..
46–48	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
49–51	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..	..	..	1	..	..	..	..	..
52–54	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
55–57	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
58–61	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1	..	..	..	..	..	..	..	..	..	..	..	..

error the average corrected times for the successive sheet of a sitting are in the relation: 107.8, 103.4, 98.4, 100.8, 99.1 96.9, 96.7, 93.8. The P. E.'s for these quantities are, i order: 2.2, 2.2, 1.5, .5, .6, .9, 1.1, 1.3. In terms of produc produced per unit of time these quantities appear as in Fig. and Fig. 3.

There is thus no evidence of "initial spurt" and very sligh evidence of "end spurt." The case is no different if onl those individuals who took three minutes or less per shee are used. The curve of work is substantially a section o the latter part of the general curve of learning plus perhap a slight "warming up" effect and a still slighter "end spurt. The irregularities toward the middle of the curve are, in m opinion, most probably a matter of accidental variation.

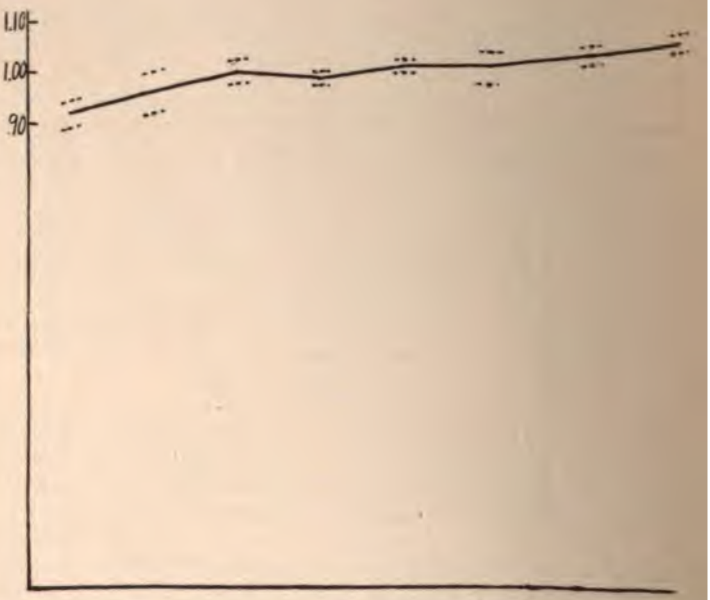

FIG. 2.—The curve of work for recording the products of pairs of num bers from a key. Equal lengths along the abscissa represent equ numbers of sheets done. The height of the continuous line repr sents the time required, as a multiple of the average time require for sheets IV and V. The dotted lines are at a distance from th continuous line equal to 2 P.E.

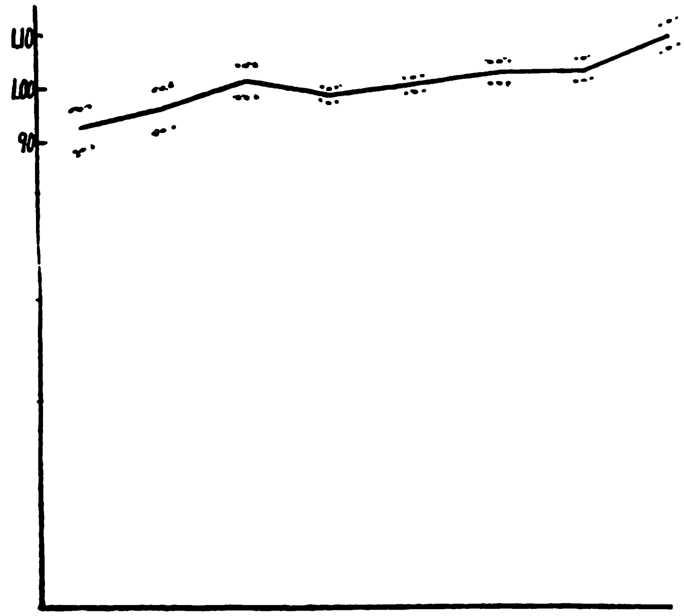

Fig. 3.—Same as Fig. 2 except that an addition of 10 sec. for each error is made.

The Correlations Between Initial Ability and Improvement and Between Improvement and One Function and Improvement in Other Functions.

Fifteen college students practiced on each of five days for each of two weeks at checking numbers on the Woodworth-Wells blank, adding columns of figures, multiplying mentally with two three-place numbers after the method used previously by the author, and typewriting. The details concerning the length of periods, the conditions of practice and the like need not be reported here. They were free from anything prejudicial to the conclusions to be stated here.[1]

The data used are shown in Table II. The six lines reported for each individual concern the results in canceling *2*'s

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[1] The practice occurred in connection with the work of the New York State Commission on Ventilation made possible by the Anderson foundation, was planned by the author and was carried on under the immediate direction of Mr. W. A. McCall. For the computations required for this article the author is responsible. Full details concerning the experiments may be found in VENTILATION IN RELATION TO MENTAL WORK by Thorndike, McCall and Chapman, New York, 1916.

(first line), canceling 3's (second line), addition (third line), mental multiplication (fourth line), and typewriting (first ten minutes in the fifth line and second ten minutes in the sixth line). The figure under 'Average' in the case of line 6 is, however, in each case, the *median* of the four scores of lines 5 and 6.

TABLE II

Initial and Final Scores of 15 Individuals

		Day 1			Day 10			Gain in Day 1 to Day 10
		Trial 1	Trial 2	Average	Trial 1	Trial 2	Average	
Brdd	2's	134	146	140	174	166	170	30
	3's	150	152	151	186	176	181	30
	Add.	94	113	104	151	149	150	46
	Mult.	50	68	59	100	98	99	40
	Typ.	155	166		222	205		
		132	158	157	219	183	212	55
Ferg.	2's	132	134	133	164	162	163	30
	3's	132	132	132	158	162	160	28
	Add.	62	79	71	122	131	127	56
	Mult.	44	37	41	90	86	88	49
	Typ.	61	77		141	144		
		78	82	78	133	151	143	65
Leik.	2's	120	130	125	154	156	155	30
	3's	142	138	140	180	156	168	28
	Add.	63	69	66	95	99	97	31
	Mult.	69	122	96	173	143	158	62
	Typ.	80	89		150	141		
		80	95	85	153	145	148	63
Levy	2's	94	110	102	136	154	145	43
	3's	98	124	111	148	142	145	34
	Add.	40	44	42	60	70	65	23
	Mult.	31	57	44	109	113	111	67
	Typ.	71	86		141	124		
		77	92	82	145	135	138	56
Bost	2's	104	106	105	200	196	198	93
	3's	124	124	124	176	176	176	52
	Add.	38	43	41	58	67	63	22
	Mult.	68	96	82	282	279	281	199
	Typ.	120	147		240	237		
		128	139	134	220	206	229	95
Elle	2's	108	108	108	195	202	199	91
	3's	108	118	113	143	166	155	42
	Add.	74	75	75	123	105	114	39
	Mult.	67	80	74	226	250	238	164
	Typ.	205	189		242	230		
		198	191	195	223	229	230	35

TABLE II—*Continued*

INITIAL AND FINAL SCORES OF 15 INDIVIDUALS

		Day 1			Day 10			Gain in Day 1 to Day 10
		Trial 1	Trial 2	Average	Trial 1	Trial 2	Average	
Solo. . . .	2's. . .	134	140	137	190	198	194	57
	3's. . .	144	150	147	196	182	189	42
	Add .	63	68	66	122	113	118	52
	Mult.	89	113	101	161	164	163	62
	Typ .	59	114		170	160		
		104	130	109	177	195	174	65
Kuen . . .	2's. . .	110	112	111	152	158	155	44
	3's. . .	122	126	124	138	142	140	16
	Add .	53	50	52	97	108	103	51
	Mult.	82	80	81	192	188	190	109
	Typ .	76	86		139	133		
		75	80	78	126	137	135	57
Rivl. . . .	2's. . .	118	122	120	184	188	186	66
	3's. . .	138	136	137	172	168	170	33
	Add .	49	44	47	106	114	110	63
	Mult.	40	47	44	179	200	190	146
	Typ .	95	118		155	161		
		101	123	110	153	174	158	48
Stac. . . .	2's. . .	88	106	97	154	166	160	63
	3's. . .	126	128	127	162	164	163	36
	Add .	79	88	84	128	124	126	42
	Mult.	65	82	74	218	212	215	141
	Typ .	39	53		125	147		
		50	66	52	125	145	135	83
Zuck. . . .	2's. . .	104	114	109	140	160	150	41
	3's. . .	122	126	124	160	152	156	32
	Add .	53	59	56	113	110	112	56
	Mult.	71	107	89	323	342	333	244
	Typ .	84	86		162	169		
		71	85	85	160	165	164	79

Except in the case of typewriting, the gain in product produced per unit of time is a little greater for those of initially high ability. In typewriting, the opposite is the case. This is what would be expected in view of the form of the curve of practice in typewriting, and the fact that the amount of practice one has had in it is largely independent of his ability.

The data of Table I give some information concerning the relation of ability to improve in one mental function to ability to improve in other functions. The correlations by the formula $r = 2 \sin \left(\frac{\pi}{6}\rho\right)$ where $\rho = 1 - \frac{6\Sigma D^2}{N(n^2-1)}$ are:

Gain in canceling with gain in adding.................. r =.10
" " " " " " mental multiplication..... r =.29
" " " " " " typewriting.............. r =.10
" " adding with gain in mental multiplication........ r =.25
" " " " " " typewriting................ r =.12
" " mental multiplication with gain in typewriting... r =.07

These correlations are subject to attenuation but not to any large extent, the correlation between the gain in canceling 2's and the gain in canceling 3's being .77, that between the gain in addition (trial 1) and gain in addition (trial 2) being .84, that between gain in mental multiplication (trial 1) and between gain in mental multiplication (trial 2) being .81, and that between gain in typewriting (first period) and typewriting (second period) being .80. It is unlikely that these 15 subjects would then with perfect measures of gain show correlations over 20% higher than the raw correlations given above. We may estimate these corrected coefficients as:

Gain in canceling with gain in adding.................. r =.12
" " " " " " mental multiplication..... r =.35
" " " " " " typewriting.............. r =.12
" " adding with gain in mental multiplication........ r =.30
" " " " " " typewriting................ r =.14
" " mental multiplication with gain in typewriting... r =.09

The probable errors due to the small number of cases are of course large, approximating .15, but it must be remembered that further cases would be as likely to decrease as increase the correlations.

The capacity to learn thus appears to be specialized in much the same way as the abilities found at any stage of learning. The lack of correlation found among the latter is not the result chiefly of differences in the relative amounts of practice which they have had, but is in very large measure more fundamental, due to characteristics of the person's original nature.

The Effects of a Day of Study and a Night of Rest Upon the Ability to Read

It is desirable to repeat the experiments that have been made upon the effects of work and rest, using ordinary tasks instead of the special work in computation, memorizing and the like, which, tho convenient for measurement, may be specially stimulating by its novelty or by the obviousness of its standards of achievement.

As one such experiment, I have used the task of reading a paragraph and answering questions about it. Twelve paragraphs, N, O, P, Q, R, S, T, V, W, X, Y and Z, were used, Z being shown here as a sample. They differed in difficulty, some being much harder than Z; and some, easier. Twelve individuals read these paragraphs and answered these questions, beginning work at or near 8 P. M., doing ten as a continuous task, and doing two more the next forenoon after a full night's rest. One individual did them in the order, N, O. P. Q, etc.; another in the order O, P, Q, R, etc.; another in the order P, Q, R, S, etc., so that each paragraph was done as the first, the second, ... the twelfth.

Z

Write your name here ....

Read this paragraph and then write the answers to questions 1, 2, 3 and 4. Read it again as often as you need to.

Certain anthropologists have been led to the conclusion that the types of human culture represent an evolutionary series; that the primitive tribes of our times represent an older stage of cultural development through which the more advanced types passed in earlier periods. An important theoretical consideration has shaken our faith in the correctness of the evolutionary theory as a whole. It is one of the essential traits of this theory that, in general, civilization has developed from simple forms to complex forms, and that extended fields of human culture have developed under more or less rationalistic impulses. Of late years we are beginning to recognize that human culture does not always develop from the simple to the complex, but that in many aspects two tendencies intercross,—one from the complex to the simple, the other from the simple to the complex. It is obvious that the history of industrial development is almost throughout that of increasing complexity. On the other hand, human activities that do not depend upon reasoning do not show a similar type of evolution. It is perhaps easiest to make this clear by the example of language, which in many respects is one of the most important evidences of the history of human development. Primitive languages are, on the whole, complex. Minute differences in point of view are given expression by means of grammatical forms; and the grammatical categories of Latin, and still more those of modern English, seem crude when compared to the complexity of psychological or logical forms which primitive languages recognize, but which in our speech are disregarded entirely. On the whole, the development of languages seems to be such that the nicer distinctions are eliminated, although it must be acknowledged that opposite tendencies are not by any means absent.

1. In what feature is the development of man's work with tools contrasted with the development of his work with words? ....

....

2. According to the doctrine that the different ways of living of different tribes of men form a progressive developmental series, what is the relation of simplicity and complexity to the temporal order of this developmental series? ....

....

3. What principle of development is almost universally characteristic of the history of industry for the past ten thousand years or more? ....

....

4. In what feature of civilization do primitive tribes seem to show greater elaborateness and delicacy of distinctions than modern Europeans? ....

....

The time spent, the wrong answers given and a rough estimate of the satisfyingness of the work in each period, were recorded. 5 was used for an ordinary condition of enjoyment of work; 10, for the maximum of satisfyingness that an individual had ever experienced; 0, for the extreme of distaste and wretchedness.

The total times (in minutes), errors, correct responses, and reported satisfyingness at each period were as follows:

EVENING AFTER WORK

Period	1	2	3	4	5	6	7	8	9	10
Time	211.0	150.3	133.6	143.5	142.3	160.5	131.5	137.5	163.5	160.0
Errors	14	16	20	8	15	17	15	17	10	16
Correct responses	51	49	45	57	50	48	50	48	55	49
Satisfyingness	69	72.5	69	74	71.5	68.5	69.5	68.5	68	64.5

NEXT DAY, AFTER REST

Period	11	12
Time	129.0	117.3
Errors	17	14
Correct responses	48	51
Satisfyingness	77.5	75.5

The time for the first paragraph read is long, the individual adapting himself to the task and being specially cautious. After that the times increase slightly. If we call the average time for periods 2, 3, and 4, 100, that for periods 5, 6 and 7 is 101.6; and that for periods 8, 9 and 10 is 105.4. After the rest the time falls, being 84.5 on the basis of 100 for periods 2, 3 and 4. How much of these differences is to be credited to the eyes and how much to the central nervous system, remains a question. It is the writer's opinion that the major share belongs to the former. The quality of the work remains closely the same throughout, the errors for periods 2, 3 and 4 being 44; those for periods 5, 6 and 7 being 47; those for periods 8, 9 and 10 being 43; and those for periods 11 and 12 after rest being 31 (or 46½ on a just basis of comparison). The satisfyingness falls off as work progresses and increases sharply after rest. The average degree of satisfyingness in periods 2, 3 and 4 was 6.0; in periods 5, 6 and 7 it was 5.8; in periods 8, 9 and 10 it was 5.6; after rest it was 6.4.

The Effect of Rests Upon Achievement and Improvement in Difficult Mental Work

The form of work used in the experiments to be reported here was the mental multiplication of a three-place number by a two-place number, 1's or 0's being excluded from the digits of the numbers. The numbers themselves were visible throughout the multiplication. Five such examples were done without any pause save that required to write the answer, note the time at which it was written and record it. At the end of such a series of five, another series was begun either (a) at once, or (b) after 10 minutes of rest, or (c) after 20 minutes of rest. This, continued for five series or 25 examples, made one day's task. The score was the time for each example plus one-fifth of the time for each wrong figure in the answer. This was understood by the subjects and they were instructed to work for the best attainable score.

The subjects were divided into three squads, each of which did 25 examples on each of three consecutive days and 5 examples in the morning of the fourth day. Squad 1 (11 individuals) worked the first day with 0 rests, the second day with 10-minute rests and the third day with 20-minute r Squad 2 (7 individuals) worked the first day with 10-mi rests, the second day with 20-minute rests, and the third with 0 rests. Squad 3 (8 individuals) worked the day with 20-minute rests, the second day with 0 rests and third day with 10-minute rests.

To prevent the very slow individuals from having u weight in determining the conclusions, I have expressed individual's scores as a per cent of his average score for entire sixteen series (of 5 examples each). When thi done and the results for each squad are averaged, we the following results:

	Day 1					Day 2					Day 3				
Squad 1	153	140	145	151	113	99	97	92	84	76	71	73	90	83	84
Squad 2	140	128	127	126	109	94	99	97	88	95	82	83	91	82	84
Squad 3	163	145	129	138	97	102	104	100	89	87	87	82	73	71	69

When the gains are put in relation to the length of the periods, the results, as shown below, favor slightly the minute rest periods, both in immediate achievement and in effect as it remains over twenty-four hours. It would b interest to discover whether the 10-minute periods woul equally effective if filled with some other variety of w The difference over 0 rest is not sufficient to justify the as periods of inactivity in practice.

GAINS FROM THE FIRST TO THE FIFTH SERIES OF THE SAME DAY

	Day 1	Day 2	Day 3	Sur
With 0 rests.....	40	15	—2	53
" 10′ "	31	23	18	72
" 20′ "	66	—1	—13	52

GAINS FROM THE FIRST SERIES OF ONE DAY TO THE FIRST SERIES OF THE FOLLOWING DAY

	Day 1 to 2	Day 2 to 3	Day 3 to 4	Sum
With 0 rests.....	54	15	6	75
" 10′ "	46	28	15	89
" 20′ "	61	12	10	83

It may be noted that the average curve of the work of three squads shows substantially no effect of the 24-hour in vals. Those intervals, under the conditions of the experim in which they were filled by the student's ordinary activi and sleep, did as much good as they did harm by the remiss of the special activity. The final day's test, either because the special rest before it, or because of the facilitation due to knowledge that it was the last of the series, or for both causes, was much better than the last series of the day before.[2]

The results of this experiment are consistent with all similar experiments in showing a very rapid improvement in the special function concerned, such as could not occur if the chief element in the efficiency of the function were a general power of ' concentration ' which the previous lives of the subjects had improved *in toto*.

The experiments give useful data concerning initial spurt—a general tendency to maintain for a minute or so a rate of achievement which is soon abandoned. I have compared the scores for the first, second and third examples in a series after rest in the case of the eight most rapid workers, the result being an average time of 77 sec. with .426 wrong figures for the first, 94 sec. and .603 wrong for the second and 82 sec. and .441 wrong figures for the third. With six individuals who required two to three minutes per example, the averages for the first, second and third examples of a series after rest were, respectively, 128 sec. with .68 wrong figures, 138 sec. with .84 wrong figures and 141 sec. with .84 wrong figures. The superiority of the work of the first minute or two is not surely due to a general tendency to initial spurt, since a part of the difficulty of this mental multiplication is the confusion of the numbers with memories persevering from previous examples, a form of interference from which the first example of a series is obviously relatively free. In general, I have failed to find evidence of initial spurt in mental work.

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[2] 152, 138, 134, 138, 106, interval, 98, 100, 100, 84, 86, interval, 80, 79, 85, 79, 79, special interval, 66.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. Titchener and H. P. Weld

XXXII. On Cutaneous After-Images

By F. L. Dimmick

In 1897 F. H. Spindler published the results of experiments on "after-sensations of touch." [1] The irregularity of these results led us, in the following year, to repeat and extend the experiments. The experimenter was Mr. J. H. Wilson, a senior in psychology; the observers were Drs. T. L. Smith and G. M. Whipple, and Messrs. H. O. Cook, W. B. Secor, and W. H. Standring, seniors in the department. Eight weights, of 50 to 1000 gr., were applied to the skin for 5, 10 and 15 sec. Our results were in substantial agreement with those of Spindler. We found evidence, however, both objective and subjective, that the principal cause of irregularity (aside from crudeness of technique) was the response of *two* sensitive tissues (skin and underlying muscle) to the stimuli employed. *Two* after-sensations of different course and quality seem to be intermingled in this "after-sensation of touch"; and neither Spindler's original method nor the improved form of it used by Wilson enables us accurately to distinguish the two components. It has, accordingly, not seemed worth while to publish Wilson's results in detail.

Meantime M. H. S. Hayes has made a much more thorough study of the after-sensations from cutaneous stimulation.[2] As a stimulus for "punctiform" pressure, she used "rounded wooden points 1/32 inch in diameter, and von Frey hairs. These were applied, in the usual way, upon pressure spots which had been located and marked several days previous." Whether the intensity of the stimulus employed was above the limen for subcutaneous pressure does not seem to have been considered. For this reason we have repeated the work upon isolated pressure spots, using a stimulus whose intensity is known to be below the limen for subcutaneous pressure. This stimulus was the hair of a von Frey hair-aesthesiometer (length, 39 mm.; diameter, 0.18 mm.; force, 0.405 g.; tension value, 4.5 gr./mm.). The skin of the fore-arm was lightly anaesthetized; and it was found that, although the stimulus was well above the limen for the normal skin, no sensation was aroused during the anaesthesia. The aesthesiometer was applied by means of a mechanical device, which we hoped would ensure constancy and accurate timing of the stimulation. The essential parts of this device consisted of a wooden plunger working in a brass cylinder. From the side of the plunger extended a horizontal arm, which worked vertically in a slot in the cylinder. The arm carried a clamp in which the aesthesiometer was held. The apparatus was so supported over the arm of the observer that it could be moved in both directions in the horizontal. An electrical circuit was made at the instant the stimulus was applied and broken when it was removed. The observer reacted by pressing a key; and both records were written alongside of a 10 per sec. time line.

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[1] Psych. Rev., iv., 631ff.

[2] Psy. Rev. Mon. Sup. 60, 1912.

Method.—The observer was comfortably seated, with his right arm extended and resting upon a felt cushion. At his left hand was the reaction-key. He was told that his arm would be stimulated by a hair, and was instructed to hold down the key so long as he felt anything at the place of stimulation. The record showed the length of time during which the stimulus was applied, the point at which the observer felt the stimulus, the point at which he ceased to feel it, the point of arousal of the after-image (if there were an after-image), and its duration. After every stimulation the observer gave a brief introspective report of his experience. Ten pressure spots on the forearm of each observer were stimulated. These were marked by clipping away all the hairs in the region of stimulation except those at the bases of which the chosen spots lay. Only one intensity of stimulus was used. The time of stimulation was approximately 1 sec. Two observers took part in the experiments: G. J. Rich (R), and R. H. Cobb (C), both graduate students in psychology. About twenty series were taken with R, and fifteen with C.

Sources of Error.—(1) The times of stimulation were taken by a stop watch, and were not absolutely constant. A record of the exact times was kept, and the variation was found to be in the neighborhood of 12%. (2) Although every pressure spot was localized within an area of less than 1 mm., it was impossible to be certain that stimulation within that area would always produce the same impression. This uncertainty is borne out by the introspections. (3) Adaptation of the pressure spots was avoided by stimulating each one only once at a sitting.

Results.—The quantitative results for the two observers are given in the following tables.

TABLE I

OBSERVER C—69 CASES

							Report of Observer	
Spot	Total Response A.Sn.+ Int.+A.Im.	M.V. of A.Sn.	Int.+A.Im.	M.V. of Int.	A.Im.	M.V. of A.Im.	Tickle	Pressure (No Report)
1	10.88	(1.56)	8.61	(0.76)	7.66	(3.55)	5	2
5	9.92	(2.36)	6.55	(1.24)	5.26	(1.47)	2	3
6	9.54	(0.69)	8.53	(0.74)	7.37	(2.99)	5	2
4	8.72	(0.54)	7.48	(0.51)	6.75	(1.98)	3	4 (1)
8	8.68	(0.70)	7.10	(0.48)	6.48	(1.24)	6	3
2	8.54	(0.57)	7.31	(0.35)	6.58	(2.15)	3	2 (2)
3	7.17	(0.49)	6.27	(0.89)	5.37	(2.14)	2	6 (1)
7	7.06	(0.18)	6.43	(0.05)	6.22	(1.74)	0	5
10	6.72	(0.2)	5.94	(0.49)	5.30	(1.68)	0	4 (1)
9	6.48	(0.56)	5.10	(0.4)	4.50	(1.34)	2	4 (1)
	Av. A.Sn.	M.V.	Av. Int.	M.V.	Av. A.Im.	M.V.		
Summary......	1.44	(0.15)	0.78	(0.57)	6.15	(2.18)	28	35 (6)

TABLE II

OBSERVER R—71 CASES

(I) 53 Cases

Spot	Total Response A.Sn.+ Int.+A.Im.	M.V. of A.Sn.	Int.+A.Im.	M.V. of Int.	A.Im.	M.V. of A.Im.	Report of Observer	
							Tickle	Pressure
6	87.34	(12.62)	70.99	(1.8)	62.64	(13.3)	1	3
2	81.45	(13.72)	58.00	(2.91)	50.50	(8.0)	1	3
7	73.76	(11.03)	62.75	(3.4)	56.49	(11.9)	0	9
10	68.83	(18.64)	47.80	(4.2)	38.50	(16.7)	1	2
4	65.70	(0.47)	63.90	(2.9)	60.30	(22.7)	0	3
3	59.56	(1.14)	55.79	(3.7)	47.56	(25.4)	0	6
9	50.97	(9.7)	46.56	(3.6)	39.34	(18.9)	1	9
1	42.95	(3.79)	38.63	(2.91)	34.72	(19.9)	4	1
5	39.08	(2.7)	33.68	(1.67)	30.40	(20.2)	2	1
8	38.34	(14.0)	27.33	(5.6)	21.63	(10.3)	1	5
	Av. A.Sn.	M.V.	Av. Int.	M.V.	Av. A.Im.	M.V.		
Summary..	11.41	(10.45)	6.7	(3.7)	44.39	(19.44)	11	42

II. 18 Cases

Spot	A.Sn.	M.V.					T	P
5	97.6	(31.1)	.....	.....	.....	.....	1	1
6	86.9	(38.5)	.....	.....	.....	.....	0	3
9	84.8	(0)	.....	.....	.....	.....	0	1
4	65.9	(35.2)	.....	.....	.....	.....	2	2
2	65.7	(19.5)	.....	.....	.....	.....	1	3
1	58.7	(0)	.....	.....	.....	.....	1	0
8	43.5	(23.2)	.....	.....	.....	.....	0	2
7	20.6	(0)	.....	.....	.....	.....	0	1
Summary..	66.7	(28.32)	.....	.....	.....	.....	5	13

That the After-Image is the most important of the moments is evident from the fact that the order of magnitude of the After-Image follows almost exactly that of the Total Response. There are only two bad inversions for R (spots 2 and 4), and one for C (spot 5), and these occur where there is an especially high M. V. for After-Sensation or Interval or both. The orders of magnitude of the other two moments do not follow that of the Total Response.

Observer R reported pressure in a large percentage of cases and tickle in comparatively few, whereas C reported tickle in nearly half of his observations.

The times for R are very much longer than those for C. The ratios between the three moments, however, are very nearly the same for both observers; and the times of all three moments for R are nearly 8 times as long as those for C. These ratios are given in the following tables.

TABLE III

Ratios	Obs. C.	Obs. R.
$\frac{\text{Latent interval}}{\text{After-sensation}}$	0.528	0.587
$\frac{\text{After-image}}{\text{After-sensation}}$	4.27	3.89
$\frac{\text{After-image}}{\text{Latent interval}}$	7.884	6.625

TABLE IV

Ratio	After-sensation	Latent Interval	After-image
$\frac{\text{Obs. R.}}{\text{Obs. C.}}$	7.9	8.5	7.2

Conclusions.—The results we have obtained seem to agree roughly with those of Hayes, though her findings are not given in a form that permits of direct comparison. Our results show the same wide variation for each observer and between observers; and the ranges of variation are approximately the same. We found the after-sensation always present. After-images were reported in a much larger number of cases than by Hayes. This difference may be due to the difference in the stimulus employed. Whether the relation which we have found between our two observers appeared in Hayes' work it is impossible to tell, but her "quantitative individual tables" indicate that an observer's times were either all long or all short.

XXXIII. ON PERCEPTIVE FORMS BELOW THE LEVEL OF THE TWO-POINT LIMEN

By E. DE LASKI

It is clear from the results obtained by Gates[1] and Titchener[2] that certain of the perceptive patterns which occur below the level of the cutaneous two-point limen are sufficiently stable for quantitative determination. It seems desirable that the whole range of these patterns[3] be systematically explored, and the present paper records a first beginning of such exploration.

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[1] E. J. Gates, The Determination of the Limens of Single and Dual Impression by the Method of Constant Stimuli, this JOURNAL, xxvi, 1915, 152ff.

[2] E. B. Titchener, in *Proc. Amer. Philos. Soc.*, lv, 1916, 208ff.

[3] For the patterns in general see M. Foucault, L'illusion paradoxale et le seuil de Weber, 1910, 124f.

The work was done during the Summer Session of 1916. The stimuli were applied by an improved Jastrow aesthesiometer to the volar side of the right arm. A line about 8 cm. in length was marked longitudinally upon the skin, beginning at a point about 2.5 cm. above the wrist; veins and hard tissues were avoided. The points were set down at different parts of this line. (It may be said at once that the area of stimulation was too large; both observers complained that the perceptive patterns varied from part to part of the line. It is better to adopt a smaller area, and to prevent fatigue by taking the observations slowly.) The observer's arm was fixed in a rest, and the aesthesiometer was applied by hand. The observers were Dr. J. N. Curtis, and Mr. F. L. Dimmick, assistant in the department.

The plan of work was as follows. Two stimuli, below the two-point limen, were chosen, and were given with knowledge in a practice-series of 10 to 20 observations (usually the same number for each stimulus). The observer was to memorise the perceptive forms and to give them names. Thereafter the experiment proper was performed: haphazard series were made up of 20 terms (10 of each stimulus), and the observer was required to name the forms as they occurred. In all cases but one, the blunt point formed by apposition of the aesthesiometer-points was used as one member of the stimulus-pair. We hoped to compare with one another all the separations employed, but lack of time forbade. The order in which the experiments were taken is shown in the following tables; the total number of judgments indicates roughly the difficulty of establishing the particular form under observation.

OBSERVER C

Stimuli	Total number of judgments	Number of Errors		Per cent accuracy	Maximal per cent accuracy in a single series
		With lesser stimulus	With greater stimulus		
App., 40 mm.	40	..	..	100	100
App., 25	160	29	20	69	85
App., 20	20	0	2	90	90
App., 15	120	14	20	72	80
App., 10	280	54	56	61	85
10, 20	140	20	19	72	85

In the first five of these experiments C judged in terms of *line* and *spot*. The *spot* was "a fairly round one with a hard place in the middle (deep, thick place)." The 40 mm.-pattern was a line with several points in it. The remaining *lines* were of the dumb-bell kind: "a vague oval with foci quite prominent and a tight little line connecting the foci." The patterns in the sixth experiment were distinguished merely by the length of this connecting line. C's patterns were, unfortunately, blurred by the shift of the stimuli along the 8 cm.-line of stimulation; she sometimes felt additional points within (or beyond?) the dumb-bell. The distribution of errors is, nevertheless, fairly regular.

OBSERVER D

Stimuli	Total number of judgments	Number of Errors		Per cent accuracy	Maximal per cent accuracy in a single series
		With lesser stimulus	With greater stimulus		
App., 30 mm.	120	13	23	70	95
App., 25	20	3	1	80	80
App., 20	60	10	7	72	85
App., 15	220	33	29	72	95
App., 10	200	22	29	75	95

In all these experiments D judged in terms of *long* and *blunt*. The latter he described as "a pressure both cutaneous and subcutaneous, a blurry shapeless spot, dull." The *longs* varied from indefinite ovals to "just a larger and more blurry mass, perhaps greater in pressure than the blunt." D was usually fatigued at the time of the experiments, and the fatigue seems to have affected the definiteness of his reports, though not his numerical accuracy. He complained, as C did, of the change of pattern with shift of stimulation. The distribution of errors is, again, fairly regular.

Conclusions.—It seems certain that subliminal separations of the aesthesiometer-points are discriminable. This result has an obvious bearing upon the determination of limens.

Discrimination appears to be based not upon length (quantitative) but upon form (qualitative); for the percentage of accuracy does not vary with the difference between the two stimuli. Moreover, we do not find any progressive effect of practice.

The introspective reports also point to the conclusion that perceptive form is the basis of judgment. C remarked, incidentally, that repetition fatigued her, and was a hindrance rather than a help.

This preliminary work shows that the stimuli should be applied (1) within a small area, (2) with light pressure, and (3) with fairly long time-intervals (perhaps 40 to 50 sec.) between the stimulus-pairs.

We have still to determine whether the perceptive forms constitute a series, and if so whether the series is continuous or discrete; and to work out the number of just noticeable differences between the perception of 'spot' or 'point' and the two-point limen.

IS INTROSPECTION INDIVIDUAL OR SOCIAL, WITHIN OR WITHOUT?

By W. D. Wallis, University of California

When Mr. McDougall assures us[1] that all psychology is built up on analogy with the psychologist's mind, he provides no means for ascertaining whether our individual psychology happens to represent that of a normal individual. His method would give us not a psychology, but psychologies as numerous and erratic as individual minds. The ideal that rests anywhere rests nowhere.

Mr. McDougall's scientific spirit shows that he means other than he says, and that by the individual mind with which he starts he means a mind of a certain type. Indeed, he himself admits this. "Before we can give an adequate account of the individual," he says, "we must be able to describe in general terms the innate basis of the mind, in so far as it is common to all men."

If this be the key to the situation, it is the type which is important, the individual mind being of value as the basis for a psychology only in so far as it conforms to this type. If this be admitted it follows that introspection is of value, not for its uniqueness, but because it expresses conformity to a type of mind external to the introspector. He must, first of all, ascertain whether his own mind is normal; and this he cannot do unless he have already the norm by which his own mind is to be standardised. Without a common norm psychologists will scarcely find a fulcrum for their aggregate efforts or reason to hope for harmony in procedures and results, beyond the consolation that pure chance may favor their ardent efforts—a coincidence so wonderful that it could be regarded only as a law not yet understood.

How can introspection give us type of mind, seeing that such introspection is but a way of finding out whether we are of the posited type? To be valid it must be controlled by a logic from without, the logic that we apply to other minds and they, in turn, apply to us.

A priori, we have no more assurance of our own mentality than of the mentality of another; self and own mind are correlatives implying other and other minds. Even the search for our own mentality implies a mentality other than that of the searcher.

An uncontrolled introspection can be nothing else than haphazard and resultness. The introspective error can be corrected only when the within is correlated with the without. Until introspection is removed from the realm of unique individual insight and made part of an inclusive system of external tests we have no accepted criterion, no uniform method. We must think into ourselves, not look into ourselves. A looking which is not a thinking provides no corrective for its illusions. If we apply an objective test, the unique advantage which an individual has with regard to his own experience is supplemented with an opportunity to view himself as would an outsider, and to apply a rigid standard over and above the elastic introspective analysis. After all, it is the outside world that leads a man to a knowledge of his self rather than a knowledge of his self that gives him a grip upon the outside world.

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[1] Psychology (Home University Library). He is following orthodox psychology.

If his mentality be part of an integral interrelated system, analysis might begin at any part of the system and proceed to any other part of it; for if the self is given meaning by reason of the world the world is given meaning by reason of the self.

The society of which the individual is a part even in his introspective moments is not far to seek. So far as he succeeds in taking the position of an outsider and in objectifying his mental content he makes of himself an observer and of his self as observed a society; for observer and observed have no meaning unless there is some third mind for which both may be object of observation.

The external social world is part of this larger self and claimant for the ascendency. To it every personality must give heed. But not in the same way. For this social world, too, may be objectified. Once objectified it may claim the ascendency, or it may be subordinated to some purpose higher than the social, to a purpose comprising both the social and the individual. The external social world must, first of all be inherited, in order to play its rôle. It must be accepted and affirmed by the individual in order that he may respond to its beck and call. In order to be an external world of compulsion the world without must become part of the world within. And once it has been placed within it may be reorientated in the individual's system of values and transformed in the light of his purposes. The outside world is part of the individual if he is really a part of it.

BOOK REVIEW

Digest of "Alcoholism and Mental Depression." By Dr. PIERRE JANET. Read August 7, 1915, before the Academy of Moral and Political Sciences, Paris, as published in *Revue Internationale de Sociologie*, October, 1915.

Despite the many studies of Alcoholism the envisagement of the subject is not yet complete. The major portion of these studies have to do with alcohol, and against its production and consumption legal measures are proposed. But this is superficial symptomatic treatment rather than etiological. It is necessary to study the alcoholic and his malady in order to attempt a cure. It is a difficult subject, one to be approached with pity and compassion for the unfortunates.

Psychologically the alcoholic is more than a drinker of alcohol,—not a mere drunkard. Intoxication or inebriation is a rapidly developed disturbance of normal thought and conduct by absorption of alcohol—an abnormal state produced in a normal individual. The alcoholic on the contrary does not become inebriated; he is an individual who has need of alcohol to become normal, yet he may know well the danger he runs in trying to satisfy this craving, as well as the expense he incurs and the family suffering he causes. He resists and promises family and friends to quit, but to no avail, though he seek medical assistance to quit. He fails to quit simply because the demand for the drink exceeds his resistance powers. Therefore we may define an alcoholic as one who needs alcohol to become normal and with whom this need is irresistible.

Whence this need and why its irresistibility is the true moral problem of alcoholism. An incident taken from my medical experience quite accurately depicts the case. A young physician of forty years, in the last stages of alcoholism, gave to me the history of his terrible passion for alcohol:

"From my youth," said he, "I was subject to a very painful malady. Occasionally, every year or two years, I fell into a state of profound melancholy and for several months would become incapable of doing things, of speaking, and especially of making decisions. The smallest act demanded extraordinary and painful efforts. I suffered keen moral torture because of a terrible contempt for myself and a feeling of moral shame. It seemed I had become the lowest of men and would befoul any place I entered. One can scarcely imagine what one suffers in such a state. I had in vain tried various treatments. One day a group of students sought me out at one of those worst moments and took me to a university fête They dragged me along and forced me to drink almost in spite of myself. The result was extraordinary. After heavy consumption of alcoholic drinks I was not inebriated but felt myself becoming more and more normal. The veil which had covered my head was rent; it seemed as though I was reborn, that I began a new life. I was again able to talk and act, and my feeling of happiness became as exaggerated as had previously been the feeling of shame. I returned home without trouble, and was able to digest and to sleep as I long had not been able to do. I awoke cured. It was inevitable that when the horrible melancholy began again some days later that I hurriedly sought the remedy, at first with curiosity, then with frenzy. I have never since been able to check myself."

Such cases can be multiplied; and this same evolution is met in the development of other impulsions towards drugs,—opium, morphine, and even to flight, but above all to drink.

The initial pathological condition is a state of mental depression. There is no destruction or disturbance of function or faculty but simply that these functions or faculties remain in potential state without culminating in action. The old psychology was static, describing faculties *per se* and seemed to take it for granted that because a faculty existed it was able to functionize. An advancement of psychology, especially in mental diseases, is that it has become dynamic, and concerns itself more with the functionizing of faculties and with what I have previously termed *psychological tension*. The degree of this tension or the elevation of the mental plane depends not upon the number of tendencies possessed by the individuals but upon the number and degree of perfection tendencies which find fruition in functionizing. The oscillations of this tension have great importance. They explain not only a large number of mental diseases but the wide variations of sleep and sleeplessness, rest and fatigue, as well as the emotions, which are all variations of the psychological tension.

Depression is a lowering of the psychological tension much below that necessary for adaptation to the environment of the individual. This depression determines a host of very curious phenomena, of impotency of action, especially of the higher and more complex ones, of agitations, of horrible feelings of abasement, and of desperate efforts to escape this state.

Opposed to these depressions there is in these oscillations another essential phenomenon,—excitement. Excitement is essentially a rapid elevation of the psychological tension; it admits of the inversed phenomena of the precedents, that is to say, of the acts of adaptation rightly performed, and of tranquillity. The faculties functionize without distress, painful feelings make way to pleasurable ones, and there is joy, interest, confidence, independence.

The absorption of certain substances, certain poisons, among which alcohol is the best known, produces excitement. Alcoholic excitement occurs even with the normal individual, but is not very remarkable and is of short duration, because the psychological tension already high is susceptible of small heightening before drunkenness ensues. But with the depressed individual excitement raises the psychological tension to the normal level. Alcohol which disturbs speech and prevents sleep in the normal man restores speech to the timid depressed one and restores sleep to the insomnolent. Alcohol thus delivers the depressed one from horrible torture, and when he relapses puts before him temptations incomprehensible to the normal individual.

It cannot be said this is dipsomania, and not common alcoholism. Too often we make this distinction. They are of the same genus in which are many varieties. Dipsomania has been distinguished because of the periodicity of the depression. These alternations are absent in the alcoholic. Chronic depressions, light or profound, are more common than one might suppose, at least for some years, which produce by the same mechanism as does dipsomania a need for exciting drink.

It is easy to show that alcoholism and mental [illegible] originate in the same conditions. Individuals wh[illegible]tation to drink even in the midst of it will [illegible]ng an infectious disease, typhoid fever or light attack of tuberculosis, just as one becomes depressed following physical or mental overwork, or following a change of surroundings, a quarrel, a disappointment in love, etc. Alcoholism acts as do all impulsions of the depressed, it diminishes with simple and easy life and augments with the higher and more trying activities. Alcoholism, like dipsomania, is a consequence of depression. I may say without here being able to demonstrate, that the same is true of that other great scourge of France, depopulation. I have reached a conclusion which may appear strange; it is that a people who alcoholize themselves, and who have no more children, is a people attacked by a sort of collective mental disease, a very general mental depression. Infectious diseases, intoxicants, alcoholism of parents with its circle of vicious results, unwholesome habitations, undernourishment, factory life, all doubtless play important rôles. Overwork, especially mental overwork peculiar to our epoch plays a chief rôle. Philosophical ideas as to equality of men have leveled ambitions. That our system of higher education is not entirely blameless is shown by numberless pathological observations. It is a delicate subject; but let me suggest: Formerly mental tension was not high and rested in beliefs quite agreeable and easy. We now repeatedly say that everybody should apply reason to his beliefs and accept as true only what seems evidently such. This is very right and beautiful; but do all little garcons have the intellect of Descartes? This is not to condemn or regret the progress of democracy or the liberty of thought, but it is to say that they are difficult and costly advancements, paid for by overwork and depression.

Placing alcoholism and even depopulation as a result of profound mental disturbances and depression caused by a diminution of vitality and overwork should not cause us to despair of applying successful therapeutics. Alcoholism must be combatted by reducing it and rendering the use of alcohol difficult. The legal measures are good if courageously enforced; but such is difficult. Selfish interests group around alcohol and these combat the enforcement of legal measures to reduce alcohol consumption. The unfortunate alcoholics themselves because of their suffering will continue to intoxicate themselves.

Besides, it is a question if the suppression of alcohol will cure the disease. Ordinarily the suppression of one excitant simply forces the diseased to seek another. We might well regard with distrust opium, cocaine, and other powerful excitants.

We have heard discussion of the excellent results of the suppression of alcohol in Russia. In medical practice things do not happen so simply. Great emotions, great dangers, energetic acts greatly heighten the courage of depressed individuals, and war is a great excitant. At this moment there are fewer depressed in Paris than under normal conditions and many ordinarily depressed maintain themselves on a superior plane. Hence the suppression of alcohol is more easily tolerated than in calm periods. We should profit by the occasion and accept the unique benefit of a horrible war. This is the time or never to suppress absinthe, alcohol, cabarets, distilleries, and obtain quickly when it will pass unnoticed this suppression which will not obtain later without great opposition and suffering.

But this alone will not suffice. Legal steps against alcohol are but symptomatic therapeutics the complete success of which would be only contingent. Some day we must go further and deal with the fundamental evil,—mental depression. We must deal with the social (the most essential) aspects of the problem not alone with the legal. Better housing conditions, cleaner air, are factors in the therapeutics of depression and alcoholism. Sufficient nourishment of workmen and the advancement of popular hygiene promise much.

Besides physical hygiene some day we will concern ourselves with moral hygiene. In conserving social advancement overwork must be prevented. To diminish social conflicts and rapid aspirations for equality and dangerous ambitions, to inspire a certain respect for authority and reconcile the irreconcilable, liberty of thought and tranquillity of belief, these are problems closely associated with that of alcoholism and depopulation.

The advancement of psychological and social sciences will one day formulate the rules of this special hygiene of the mind and the academy of moral sciences will then be the great council of moral hygiene. In the meantime do not despair. Individual depressions frequently yield to treatment,—those of the people may be cured also.

Independence, Missouri. FREDERICK M. SMITH.

BOOK NOTES

Evolution and the war. By P. CHALMERS MITCHELL. London, John Murray, 1915. 114 p.

This is a brilliant and original book by a well-known biologist. He discusses war and the struggle for existence, this latter struggle among animals, nationality and race, the production of a nationality, and selective factors, the foundations of nationality, with epigenetic features. The main feature of the book is its rather radical denial of the existence or at least the importance of psychic heredity. Part of it he thinks is due to suggestion and that if people never knew the traits of their progenitors they would often never develop them. The very fact of educability suggests that man is far more plastic to develop in individual directions, quite apart from his ancestry, than animals are. He would not perhaps say the mind is a *tabula rasa,* but all kinds of possibilities are open. He holds that consciousness transforms all qualities and faculties acquired by human beings from the animal world, and that is the foundation of free and intelligent existence. He holds Kant responsible for what he calls "the dreaming megalomania that has destroyed the German sense of reality and has made German *Kultur* the enemy of the human race." Nietzsche was only a terminal flower of this poisonous and sterile idealism. Bernard Shaw is "only Nietzsche grinning through a horse collar." As against Darwin he urges "that the moral law is as real and external to man as the starry vault. It has no secure seat in any single man or any single nation." It is not inborn but inherited from traditions and customs in literature and religion. Its creation and subsistence is the crowning glory of man and his consciousness of it puts him in a high place above the animal world. The struggle for existence, therefore, does not apply to man. Modern nations are not units of the same order as the units of the animal and vegetable kingdom from which the law of the struggle for existence is a supposed inference. Darwin's struggle has no analogue in human welfare. Man is not subject to the laws of the unconscious and his conduct is not to be judged by them but by its harmony with a real and external not-self that man has built up through the ages.

The instincts of the herd in peace and war. By W. Trotter. London, T. Fisher Unwin, 1916. 313 p.

This book is made up partly of essays written at various times since 1908 by a surgeon interested in psychological matters, and who writes a very effective style. It is a book that should be carefully read and pondered by every psychologist, for to this science it really makes valuable and new contributions. It is in a sense a little lacking in unity, the review of Freudianism and especially the war chapters having been rather an afterthought. At the same time, back of this the book really has a fundamental unity. The author goes far beyond the writers of the Tarde and LeBon school in insisting that the herd instinct is just as fundamental as those instincts generally classed as more primitive. He does not agree with Freud that the former are well developed and then repressed by social tendencies, but believes the latter to have been primordial and makes them the key to the explanation of some of the most fundamental things in life. Having developed his main thesis, he applies it to the issues of the war by urging that the Germans represent the wolf type and the English the sheep type of the herd instinct. Dr. John T. MacCurdy has written an excellent little ten-page summary of the work in the Psychiatric Bulletin for January, 1916, and has a high appreciation of the brilliant speculations of the author, who holds that psychology is the science of the future. One cannot but wonder whether a surgeon with such strong psychological proclivities may not, in what he says about the solution of the conflict between experience and herd suggestion, write with some personal warmth.

The psychology of the negro; an experimental study. By George Oscar Ferguson, Jr. (Archives of Psychology, No. 36, April, 1916.) New York, Science Press. 138 p.

In his preliminary account of the non-experimental studies, the author does strange injustice to Odum's work, which is by far and away the best of all studies ever made in this field, compared with which the works of Tylor, Hall, Thorndike and the rest, are insignificant. Of course the author's work is experimental, but we maintain that he should have correlated in his own thought at least the very voluminous literature on the subject instead of practically having ignored it. It is by no means clear how much we can trust the maze, cancellation, sorting and other tests, and the author seems to have an exaggerated idea of the reliability of these methods. Certainly his own experiments afford him very few data for the "general comparison of whites and negroes" which he attempts. The author seems to the writer of this note bitten with the method fetichism that pervades the atmosphere in which he did his work.

How to live. By Irving Fisher and Eugene Lyman Fisk. New York, Funk and Wagnalls Co., 1916. 345 p.

This work is a rare combination of the scientific and practical. It is wrought out in collaboration with the Hygienic Board of the Life Extension Institute. It contains a brief but excellent foreword by ex-President Taft, and an account of the above Board with photographs of its leading members. It covers such points as housing, clothing, outdoor life and sleeping, deep breathing, various themes connected with food and poisons, work, play, sleep, serenity, general hygiene, its rules, unity, obstacles, relations to civilization, etc., with eight supplementary notes on such topics as over-weight, under-weight, posture, alcohol, tobacco, avoiding colds, signs of disease, constipation and degenerative tendencies, and eugenics.

The mythology of all the races. Edited by Louis Herbert Gray. *Vol. I. Greek and Roman.* By William Sherwood Fox. Boston, Marshall Jones Co., 1916. 354 p.

This opening volume of a series of thirteen which is contemplated, with its preface by Professor G. F. Moore of Harvard, makes an excellent impression in both the quality of work and the get-up of the book. It has long seemed to us that the need of some such thesaurus of mythology was great. If the series can be carried to completion on the same high grade as this initial volume it will be a godsend now that mythology is coming to have a new entire symbolic interpretation. We shall look with great interest to see what attitude the editors take with regard to the new psychoanalysis of myths.

La système Taylor et la physiologie du travail professionnel. By J. M. Lahy. Paris, Masson et Cie, 1916. 198 p.

This is an epitome of the efficiency work and ideas of the late Frederick W. Taylor which are set forth with lucidity in nine chapters. The author seems to share with little reservation Mr. Taylor's views of the economy of movement and professionalization of labor. In separate chapters he discusses salaries, efficiency in business organizations, the psychology of work and the problems of fatigue, and in the concluding chapter hails the work of Mr. Taylor as of very great significance.

The function of socialization in social evolution. By Ernest W. Burgess. Chicago, University of Chicago Press (c. 1916). 237 p.

This work is divided into three parts. The first treats of the rôle of socialization in discovery and invention, its relations to specialization, the social heritage, organization, stimuli and demand; the second treats of its rôle in social progress, from the kinship to the personal stage, including feudal, town and finally the impersonal stage of socialization. Part three treats of its rôle in personal development, its cognitive, affective and volitional aspect.

The British Journal of Psychology. Edited by Charles S. Myers. Vol. 8, Part 2. May, 1916. Cambridge, University Press.

This number contains Part Three of N. Carey's Factors in the Mental Processes of School Children; this deals with the school subjects. It also contains an interesting article by James Ward on Sensory Character of Black.

Proceedings of the Royal Society of Medicine. Vol. 9, No. 7. May 1916. London, Longmans, Green and Co.

Medical Review of Reviews. Vol. 22, No. 7. July, 19
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